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ORIGINAL ARTICLES.

THE DIRECT ACTION OF CALCIUM, SODIUM, POTASSIUM, AND AMMONIUM SALTS ON THE BLOODVESSELS.

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THE experiments of Ringer on the influence of certain calcium and potassium salts on the contractility of the frog's ventricle, have attracted well-deserved attention from the profession.

In the several papers published in the *Journal of Physiology*, he has shown that very minute quantities of potassium and calcium salts possess the power of greatly modifying the contractility of the heart. He pointed out that even so minute a quantity of potassium chloride as 1 in 15,000, and of calcium chloride as 1 in 10,000, is capable of promoting the normal cardiac contractility, provided they are brought in contact with the ventricle in the presence of a three-quarter per cent. saline medium.

Ringer, while not attempting to explain how such minute quantities of inorganic salts can influence so profoundly the organized structures of the heart, thinks it not improbable that the salts do not enter into direct combination with the molecules of the muscular tissue, or, if any combination does take place, that it is of an extremely loose character, since the salts are easily dislodged and washed away by the mere circulation of a fluid devoid of these salts.

Kronecker, and some of his pupils, find that if a ventricle is fed with a 0.6 per cent. normal saline solution, contractility soon ceases, and a strong induction-shock will not excite contractions. They conclude that the suspension of contractility depends on the saline solution washing out all the nutritive material from the cavities of the heart and the meshes of its spongy substance.

Merunowicz (quoted by Ringer) found that the dissolved ash of incinerated blood furnishes a good fluid for supporting the heart's contractility, and Gaule concluded that the essential agent in the ash which sustains the contractility is an alkaline carbonate. Gaule also found that the addition of an alkaline sodium salt, even in small quantity, to saline solution, enables it to sustain contractility longer than when simple saline solution is used.

Gaule, therefore, differs from Kronecker, and maintains that the heart can feed on its own tissues, and derive from them all the materials necessary for a contraction. Kronecker, on the other hand, insists that the frog's heart does not feed on its own substance, and that as soon as nutritive fluid is withdrawn its contractility ceases.

We think that Ringer has shown conclusively that certain alkaline salts will prolong the contractility of the heart, and calcium salts prolong it more than sodium salts. Ringer very correctly observed that the character of the contractions underwent much alteration under the influence of these salts, but that the addition to the circulating fluid of a physiological quantity of a potassium salt entirely obviated these alterations, adding greatly to the duration of the contractility. It was found that while calcium salts promoted the contractility, potassium salts favored the relaxation of the ventricle.

Thus, by adding to normal saline solution certain small quantities of bicarbonate of sodium, chloride of potassium, and chloride of calcium, Ringer finally succeeded in obtaining a circulating medium by means of which normal contractility could be maintained for hours.

On looking over the literature of the subject, I found that very little indeed had been done to ascertain the nature of the action of these salts on the vascular system. The interest and importance which attaches to the results of Ringer's experiments on the heart made it appear very desirable that something more should be done with a view of ascertaining the exact influence of these salts on the vascular system.

With this object in view, the experiments which follow were made.

On account of the great scarcity of frogs in midwinter these experiments were all made with terrapins. My method consisted in cutting out the heart and inserting canulas into the aortic trunks, and one canula into the sinus. The aortic canulas were connected with Mariotte's flasks, placed at a certain height above the body of the animal, from which flasks liquid was made to flow into the arteries. This liquid, after circulating through the capillaries and veins, was drained off from the sinus into a beaker, and measured at definite intervals. After this outflow had become constant, circulating fluid, plus the drug, was substituted for the normal circulating fluid, and the action of the drug on the vessels determined from the change which took place in the amount of the outflow in a unit of time. A decreased outflow indicated increased resistance to the flow of the fluid through the vessels, or, in other words, a constriction of them, and an increased outflow, a dilatation of them.

In most of my published experiments on the action of drugs on the heart and bloodvessels, I made use of a circulating fluid which I termed "Ringer's saline," on account of his having first proposed it as a good substitute for the normal salt solution wherever the use of the latter might be indicated. As a diluent for mammalian blood to be used in experiments on the isolated heart of cold-blooded animals, it certainly was found to surpass by far the ordinary seventy-five one-hundredths per cent. salt solution in keeping the heart active and alive to the action of drugs as well as to other influences.

But in experimenting with different alkaline salts on the bloodvessels, I was naturally obliged to fall back upon normal salt solution as the standard, and, therefore, the first experiments were made with a view of ascertaining the influence on the bloodvessels of Ringer's saline as compared with that of normal salt solution.

EXPERIMENT XLI.—March 23, 1886. Terrapin 1250 grms. Inflow canulas in right and left arterial trunks. Outflow canula in sinus. Arterial pressure 18 c. m. Venous pressure 0. Circulating fluids: Ringer's saline and normal salt solution (0.75 per cent.).

Time from	P. M. to	Temp. Cent.	Total outflow.	Outflow per min.	Circulating fluids were supplied to the vessels at the time mentioned on same line in second column.
3.14	3.16	16°	49	24.5	On normal salt solution.
16	18	...	50	25	
18	20	...	49	24.5	
20	22	...	52	26	
22	24	16	52	26	
24	26	...	52	26	On Ringer's saline.
26	28	...	53	26.5	
28	30	...	52	26	
30	32	16	26	13	
32	34	...	30	15	
34	36	...	30	15	
36	38	...	32	16	
38	40	16	30	15	
40	42	...	29	14.5	On normal salt solution.
42	44	...	20	10	
44	46	...	24	12	
46	48	16	26	13	
48	50	...	26	13	
50	52	...	28	14	
52	54	...	30	15	
54	56	16	32	16	
56	58	...	35	17.5	
58	4.00	...	37	18.5	
4.00	02	...	38	19	
02	04	...	38	19	
04	06	...	40	20	
06	08	16	40	20	On Ringer's saline.
08	10	...	35	17.5	
10	12	...	30	15	
12	14	...	21	10.5	
14	17	16	31	10.3	
17	19	...	21	10.5	
19	21	...	21	10.5	
21	23	...	20	10	
23	25	16	18	9	On normal salt solution.
25	27	...	16	8	
27	29	...	19	9.5	
29	31	...	22	11	
31	33	16	25	12.5	
33	35	...	30	15	
35	37	...	32	16	
37	39	...	34	17	
39	41	16	38	19	
41	43	...	40	20	
43	45	...	42	21	Ended.

Experiment XLI. is one out of the three experiments made with regard to the influence of Ringer's saline on the bloodvessels as compared with that of normal salt solution. They all show very clearly that Ringer's saline possesses over normal salt solution the power of greatly contracting the bloodvessels. This contraction, as shown in this experiment, takes place gradually, but the dilatation which follows the readmission of normal salt solution is very slow.

The composition of this fluid is as follows:

Normal salt solution . . .	(0.75 per cent.)	100 c. c.
Calcium chloride solution .	(1 : 390)	5 "
Sodium bicarbonate solution	(0.50 per cent.)	2.5 "
Potassium chloride solution	(1.0 per cent.)	0.75 "

Mix and filter.

The question naturally arose as to which one of the different constituents of this fluid gave rise to the contraction of the vessels. Several experiments were made with regard to this point, but Experiment XLII. will suffice to show that it was the chloride of calcium. As indicated in the heading, three different circulating fluids were made use of: First, normal salt solution; second, normal salt solution to which had been added potassium chloride in the exact proportion in which this was contained in Ringer's saline; third, Ringer's saline, the potassium chloride left out. Potassium chloride, therefore, was used in extremely small quantities, the solution containing less than 1 : 10,000.

This experiment shows very clearly that normal salt solution and potassium chloride give rise to great dilatation of the bloodvessels, while, on the other hand, Ringer's saline without potassium chloride causes their contraction. As may be seen in the experiment, the vessels, contracted by Ringer's saline minus potassium chloride, are gradually dilated by normal salt solution, and this dilatation is still further increased upon the admission of normal salt solution plus potassium chloride. Ringer's saline now readmitted, within a very few minutes may be noticed to reduce the quantity of the outflow to one-third its former amount, therefore, also reducing the calibre of the vessels in about the same proportion; normal salt solution reëstablished the normal calibre of these vessels, while potassium chloride, again, caused them to dilate.

From this and other experiments, all concordant, it is plain that everything pointed to the chloride of calcium as being the cause of the contraction of the vessels, and, at the same time, to the potassium chloride as causing their dilatation. Both having been used in the same proportion in which they are contained in Ringer's saline—that is, in very minute quantities—we may conclude (1) that calcium chloride, even in very small doses, gives rise to contraction, and potassium chloride in equally minute doses to dilatation of the bloodvessels in the terrapin.

EXPERIMENT XLII.—March 24, 1886. Terrapin 990 grms. Inflow canulas in right and left arterial trunks. Outflow canula in sinus. Arterial pressure 18 c. m. Venous pressure 0. Circulating fluids: (*a*) normal salt solution, (*b*) normal salt solution + potassium chloride, (*c*) Ringer's saline — potassium chloride.

Time from	P. M. to	Temp. Cent.	Total outflow.	Outflow per min.	The circulating fluids were supplied to the vessels at the time mentioned on the same line in the second column.
3.07	3.09	16°	28	14	Normal salt solution(*a*).
09	11	...	30	15	
11	13	...	30	15	
13	15	16	29	14.5	
15	17	...	31	15.5	Normal salt solution + potas-
17	19	...	35	17.5	[sium chloride(*c*).
19	21	...	48	24	
21	23	16	48	24	
23	25	...	50	25	
25	27	...	45	22.5	
27	29	...	35	17.5	
29	31	16	31	15.5	
31	33	...	30	15	
33	35	...	32	16	
35	37	...	32	16	On normal salt solution(*a*).
37	39	16	36	18	
39	41	...	40	20	
41	43	...	42	21	
43	45	...	47	23.5	

Time from P. M.	to	Temp. Cent.	Total outflow	Outflow per min.	The circulating fluids were supplied to the vessels at the time mentioned on the same line on the second column.
45	47	16	46	23	
47	49	...	48	24	
49	51	...	48	24	
51	53	...	48	24	On normal salt solution +
53	55	16	52	26	[potassium chloride(*c*).
55	57	...	62	31	
57	59	...	62	31	
59	4.01	...	63	31.5	
4.01	03	...	67	33.5	On Ringer's saline — potas-
03	06	16.5	76	25.3	[sium chloride(*b*).
06	08	...	36	18	
08	10	...	24	12	
10	12	...	22	11	
12	14	...	23	11.5	
14	16	16.5	23	11.5	
16	18	...	22	11	On normal salt solution(*a*).
18	20	...	26	13	
20	22	...	30	15	
22	24	...	32	16	
24	26	16.5	30	15	
26	28	...	31	15.5	On normal salt solution +
28	30	...	36	18	[potassium chloride(*c*).
30	32	...	42	21	
32	34	16.5	48	24	
34	36	...	54	27	[slight œdema.
36	38	...	60	30	Observations discontinued;

In order to test still further the action of lime salts on the vessels, a saturated solution of sulphate of lime was made, and the solution converted into normal salt solution by simply adding the required amount of chloride of sodium. In addition, a solution of bromide of potassium was made containing one gramme of the bromide in 3000 cubic centimetres of normal saline solution. Several observations were made with the solution of bromide of potassium, but since they were all concordant, one will here suffice to show the effect it produces on the vessels.

Experiment XLV. clearly shows the influence of both calcium and potassium salts on the vessels in larger doses than had been used in previous experiments. It was found that under sulphate of lime the quantity of the outflow was gradually diminished to about one-fifth normal, and that, therefore, the calibre of the vessels must have become reduced in about the same proportions. Bromide of potassium, on the other hand, gave rise to an increased outflow, hence must have dilated the vessels.

The conclusions, therefore, are obvious, namely, calcium salts, in comparatively large doses, cause contraction, and potassium salts, in medium doses, cause dilatation of the bloodvessels, in the case of the terrapin.

EXPERIMENT XLV.—March 27, 1886. Terrapin 460 grms. Inflow canulas in right and left aortic trunks. Outflow canula in sinus. Arterial pressure 16 c.m. Venous pressure 0. Circulating fluids: normal salt solution, saturated solution of calcium sulphate made into normal salt solution (0.75 per cent.), normal salt solution and bromide of potassium 3000 c. c. : 1.

Time from P. M.	to	Temp. Cent.	Total outflow.	Outflow per min.	
3.06	3.08	16°	28	14	Normal salt solution.
08	10	...	28	14	
10	12	...	29	14.5	
12	14	...	28	14	
14	16	...	29	14.5	On calcium sulphate solution
16	18	16	24	12	
18	20	...	21	10.5	
20	22	...	14	7	
22	24	...	8	4	On normal salt solution.
24	26	...	7	3.5	
26	28	...	7	3.5	
28	31	16	12	4	
31	33	...	12.5	6.2	
33	36	...	31	10.3	
36	38	...	23	11.5	
38	40	...	27	13.5	
40	42	...	28	14	On calcium sulphate solution.
42	44	16	26	13	
44	46	...	21	10.5	
46	48	...	13	6.5	
48	50	...	11	5.5	On normal salt solution.
50	53	...	13	4.3	
53	55	...	8	4	
55	57	...	11	5.5	
57	59	...	13	6.5	
59	4.01	...	15	7.5	
4.01	03	...	18	9	
03	05	...	20	10	
05	08	...	31	10.3	
08	10	...	24	12	
10	12	...	25	12.5	On solution of bromide of
12	15	...	39	13	[potassium.
15	17	...	28	14	
17	20	...	46	15.3	
20	22	...	35	17.5	
22	24	...	36	18	
24	26	...	41	20.5	[considerable œdema.
26	28	...	48	24	Observations discontinued;

EXPERIMENT L.—April 1, 1886. Terrapin 1205 grms. Inflow canulas in right and left aortic trunks. Outflow canula in sinus. Venous pressure 0. Arterial pressure 14 c. m. Circulating fluids: Normal salt solution, solution of sodium iodide in normal salt solution 1 : 1000, solution of sodium bromide in same proportion.

Time from P. M.,	to	Temp. Cent.	Total outflow.	Outflow per min.	Circulating fluids were supplied to the vessels at the time mentioned on the same line in the second column.
2.55	2.57	18°	18	9	On normal salt solution.
57	59	...	18	9	
59	3.01	...	18	9	
3.01	03	...	18	9	
03	05	18	19	9.5	
05	07	...	18	9	On sodium iodide solution.
07	09	...	20	10	
09	11	...	26	13	
11	13	...	32	16	
13	15	18	24	12	
15	17	...	16	8	
17	19	...	10	5	On normal salt solution.
19	21	...	8	4	
21	23	...	10	5	
23	25	18	15	7.5	
25	27	...	14	7	
27	29	...	14	7	
29	31	...	14	7	
31	33	...	14	7	
33	35	18	14	7	On sodium iodide solution.
35	37	...	15	7.5	
37	39	...	16	8	
39	41	...	16		
41	43	...	18		
43	45	...	22		
45	47	18.5	26	...	On normal salt solution.
47	49	...	14		
49	51	...	13		
51	53	...	18		
53	55	...	19		
55	57	18.5	18	...	On sodium bromide solution.
57	59	...	18		
59	4.01	...	18		
4.01	03	...	32		
03	05	...	36		
05	07	18.5	34		
07	09	...	24		
09	11	...	20		
11	13	...	12		
13	15	...	10	...	On normal salt solution.
15	17	...	12		
17	19	18.5	15		
19	21	...	15		
21	23	...	14		
23	25	...	14	...	On sodium bromide solution.

Time P. M. from	to	Temp. Cent.	Total outflow.	Outflow per min.	Circulating fluids were supplied to the vessels at the time mentioned on the same line in the second column.
25	27	...	15		
27	29	18.5	22		
29	31	...	28		
31	33	...	33		
33	35	...	38		
35	37	18.5	28		
37	39	...	24	...	On sodium iodide solution.
39	41	...	20		
41	43	...	15		
43	45	...	13		
45	47	...	10		
47	49	18.5	11		
49	51	...	10		
51	53	...	9		
53	55	...	9	...	On normal salt solution.
55	57	...	5.5		
57	59	...	6		
59	5.03	...	16		
5.03	07	...	26		
07	09	...	12		
09	11	...	14		
11	13	...	14		
13	15	19	15	...	Observations discontinued.

The observations recorded in Experiment L. show that bromide and iodide of sodium in large doses give rise to dilatation of the bloodvessels, which dilatation is followed by contraction of the same. The same result was obtained when these salts were used in the proportion of 1 : 5000, with the exception that the dilatation produced lasted longer, and the contraction came on much later, and was much more gradual. Sodium salts, therefore, differ materially from those of calcium and potassium in their effects upon the bloodvessels.

EXPERIMENT XLVII.—March 30, 1886. Terrapin 1370 grms. Inflow canulas in right and left aortic trunks. Outflow canula in sinus. Arterial pressure 16 c. m. Venous pressure 0. Circulating fluids: Normal salt solution, sodium bicarbonate in normal salt solution 1 : 1000, potassium iodide in normal salt solution 1 : 1500.

Time P. M. from	to	Temp. Cent.	Total outflow.	Outflow per min.	
2.40	2.42	16°	20	10	On normal salt solution.
42	44	...	20	10	
44	46	...	20	10	
46	48	...	21	10.5	
48	50	...	22	11	On sodium bicarbonate solution.
50	52	16	24	12	
52	54	...	28	14	
54	56	...	35	17.5	
56	58	...	41	20.5	On normal salt solution.
58	3.00	...	33	16.5	
3.00	02	16	24	12	
02	04	...	24	12	On sodium bicarbonate solution.
04	07	...	40	13.3	
07	09	...	32	16	
09	11	16	28	14	
11	13	...	22	11	
13	15	...	20	10	
15	17	...	16	8	
17	20	...	18	6	
20	23	...	12	4	On normal salt solution.
23	26	...	14	4.6	
26	28	16	11	5.5	
28	30	...	14	7	
30	32	...	16	8	On potassium iodide solution.
32	34	...	16	8	
34	36	...	19	9.5	
36	38	16.5	21	10.5	
38	40	...	23	11.5	
40	42	...	24	12	
42	44	...	26	13	
44	46	...	28	14	
46	48	...	30	15	
48	50	16.5	35	17.5	
50	52	...	36	18	
52	54	...	40	20	On normal salt solution.
54	56	...	33	16.5	
56	58	...	29	14.5	
58	4.00	...	27	13.5	
4.00	02	17	20	10	
02	04	...	20	10	On potassium iodide solution.
04	06	...	24	12	
06	08	...	26	13	
08	10	...	29	14.5	
10	12	...	31	15.5	
12	14	...	33	16.5	
14	16	17	36	18	
16	18	...	42	21	[œdema.
18	20	...	48	24	Observations discontinued;

Experiment XLVII. shows that bicarbonate of sodium, like bromide and iodide of sodium, causes dilatation of the bloodvessels, which dilatation is followed by a contraction; it shows, furthermore, that iodide of potassium, like chloride of potassium, gives rise to a dilatation of the bloodvessels, which dilatation, even when very large doses are used, is permanent, and at no time followed by contraction. We may, therefore, safely conclude that sodium salts give rise to dilatation followed by contraction of the bloodvessels, and that potassium salts, in large as well as small doses, always cause dilatation of the vessels.

In the next experiment the bromide of potassium was used in the proportion of 1 of the bromide to 3000 cubic centimetres of normal salt solution, or about half the strength of the potassium iodide solution used in previous experiments. The effect on the bloodvessels may be seen to be identical with that produced by the iodide, thus showing that it is not due to the iodine or bromine in combination with potassium, but rather to the potassium part of these salts. This may also be inferred from the fact that the chloride of potassium, too, produces the same effect upon the bloodvessels, namely, it causes their dilatation in small as well as large doses.

In connection with the dilatation of the vessels produced by potassium salts, considerable œdema was invariably noticed; this was absent in experiments with calcium and sodium salts.

EXPERIMENT XLIV.—March 26, 1886. Terrapin 650 grms. Inflow canulas in right and left aortic trunks. Outflow canula in sinus. Arterial pressure 18 c.m. Venous pressure 0. Circulating fluids: Normal salt solution, solution of potassium bromide 1 in normal salt solution 3000.

Time P. M. from	to	Temp. Cent.	Total outflow.	Outflow per min.	
3.08	3.10	18°	11	5.5	Normal salt solution.
10	12	...	11	5.5	
12	14	...	10	5	
14	16	...	10	5	
16	18	...	12	6	
18	20	...	12	6	
20	24	18	24	6	On potassium bromide solution.
24	26	...	14	7	
26	28	...	17	8.5	
28	30	...	18	9	
30	32	...	24	12	
32	34	...	30	15	On normal salt solution.
34	36	18	18	9	
36	38	...	16	8	
38	40	...	11	5.5	
40	42	...	11	5.5	On potassium bromide solution.
42	44	...	13	6.5	
44	46	...	17	8.5	
46	48	18	24	12	
48	50	...	30	15	On normal salt solution.

Time P. M. from	to	Temp. Cent.	Total outflow.	Outflow per min.	
50	52	...	24	12	
52	54	...	13	6.5	
54	56	...	9	4.5	
56	58	...	9	4.5	
58	4.00	...	10	5	
4.00	02	...	10	5	On potassium bromide solu-
02	04	...	12	6	[tion.
04	06	...	15	7.5	
06	08	...	18	9	
08	10	...	23	11.5	
10	12	...	29	14.5	On normal salt solution.
12	14	...	16	8	
14	16	...	14	7	
16	18	...	10	5	
18	20	...	9	4.5	
20	22	...	8	4	
22	24	...	9	4.5	[great œdema.
24	26	19	10	5	Observations discontinued;

Experiment XLVIII. has been selected from three experiments made with ammonium salts on the bloodvessels, because it represents in a typical manner what took place in all, namely, great dilatation followed by slight contraction; the latter, though rarely observed as coming on during the time the ammonium salt solution is in the bloodvessels, must nevertheless be looked upon as an effect of ammonium salts, for the vessels ultimately recover their normal calibre under the influence of normal salt solution. Hence, the salts of ammonium resemble those of sodium in their effects on the bloodvessels.

EXPERIMENT XLVIII.—March 31, 1886. Terrapin 1210 grms. Inflow canulas in right and left arterial trunks. Outflow canula in sinus. Arterial pressure 12 c.m. Venous pressure 0. Circulating fluids: Normal salt solution — chloride and bromide of ammonium in normal salt solution, each in the proportion of 1 : 1000.

Time P. M. from	to	Temp. Cent.	Total outflow.	Outflow per min.	
2.55	2.57	16°	30	15	Normal salt solution.
57	59	...	30	15	
59	3.01	...	31	15.5	
3.01	03	...	30	15	
03	05	...	30	15	
05	07	...	29	14.5	
07	09	16	29	14.5	On ammonium chloride solu-
09	11	...	29	14.5	[tion.
11	13	...	37	18.5	
13	15	...	42	21	
15	17	...	48	24	On normal salt solution.
17	19	...	38	19	
19	21	...	18	9	
21	23	...	20	10	
23	25	...	25	12.5	
25	27	16	30	15	
27	29	...	30	15	
29	31	...	32	16	On ammonium chloride solu-
31	33	...	40	20	[tion.
33	35	...	59	29.5	
35	37	...	74	37	On normal salt solution.
37	39	...	61	30.5	
39	41	...	45	22 5	
41	43	...	42	21	
43	45	...	41	20.5	
45	47	...	42	21	
47	49	...	41	20.5	On ammonium bromide.
49	51	...	46	23	
51	53	...	66	33	
53	55	...	78	39	On normal salt solution.
55	57	...	60	30	
57	59	...	55	27.5	
59	4.01	...	50	25	
4.01	03	...	38	19	
03	05	...	28	14	
05	07	..	36	18	
07	09	...	35	17.5	On ammonium bromide.
09	11	...	45	22.5	
11	13	...	90	45	On normal salt solution.
13	15	...	73	37.5	
15	17	...	58	29	
17	19	...	52	26	
19	21	...	48	24	
21	23	...	41	20.5	[no œdema.
23	25	...	41	20.5	Observations discontinued:

The amount of chloride and bromide of ammonium used in this experiment was larger than that used in the remainder of them. The effect, however remained the same. The dilatation produced by ammonium salts, in large or small doses, is prompt and extensive.

With the exception of Ringer's experiments, referred to above, very little indeed is found in literature on the physiological characteristics of calcium salts. The same may be said of the salts of sodium so far as their effect on the circulatory apparatus is concerned.

Potassium salts have been found to cause death by heart-paralysis, being both nerve and muscle poisons. The primary effect of the potassium salts on the circulation is said to be a transient decrease in blood-pressure, together with a slowing of the pulse-rate, but a rise in pressure and an increase in the rate quickly follow. The rate, however, again begins to go down while the pressure may still be on the rise. Finally, large doses have been found to cause immediate and permanent lowering of blood-pressure, as well as slowing of the pulse-rate, which may culminate in diastolic arrest.

Drs. Lauder Brunton and Cash, in their experiments with potassium salts on the bloodvessels, found that potassium chloride gave rise to great contraction, which exceeded even that produced by calcium and strontium salts. This certainly is not the *direct* action of these salts in the case of the terrapin.

The salts of potassium, as is well known, are frequently employed by practitioners in combination with digitalis for the diuretic effects which they produce. If, now, they did produce dilatation of the renal vessels in man as they do in the terrapin, which, I think, is more than probable from the foregoing experiments, their combination with cardiac tonics like, for instance, digitalis, would certainly go far to explain their salutary diuretic effects which they are known to produce in dropsies of all kinds.

According to Funke and Deahne, the salts of ammonium cause temporary contraction of the bloodvessels owing to their stimulating influence upon the vasomotor centres in both brain and spinal cord; the same authors also state that these salts possess a stimulating influence on the inhibitory centre in the brain, thereby causing slowing in the pulse-rate, in larger doses, diastolic cardiac arrest.

In conclusion, we offer the following explanations for the phenomena observed in the foregoing experiments:

1. Calcium salts cause the vessels to contract by virtue of their stimulating influence on the vasomotor ganglia.

2. Sodium and ammonium salts excite, first, the ganglia of the vasodilators; next, those of the vaso-

motors; hence producing at first dilatation, and afterward contraction of the vessels.

3. Potassium salts stimulate the ganglia of the vasodilators only, and consequently produce dilatation; if, however, as was shown in two observations, the dilatation which they produce is followed by contraction, this contraction is so extremely slight that it may practically be neglected; therefore, any stimulating influence on the vasomotor ganglia which they might possess is insignificant when compared with that which they exert over the vasodilators.

THE AMBLYOPIA OF SQUINTING EYES: IS IT A DETERMINING CAUSE, OR A CONSEQUENCE OF THE SQUINT?[1]

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THE question whether the amblyopia which exists in the squinting eye in so large a proportion of cases of concomitant convergent strabismus is a consequence of the squint, or whether it antedates it, and is an important factor in determining its development, is one not of mere theoretical interest, but of practical importance; for upon its solution depends the further question, whether the operative correction of this deformity shall be undertaken at the earliest moment possible after it has become established, or whether it may be postponed, without detriment to the result, to a later period of life. If the views of Schweigger and of Alfred Gräfe, that the amblyopia is a congenital defect, and not a result of the squint, be correct, it is a matter of little moment when the operation shall be undertaken; but, on the other hand, if, as was commonly held before the promulgation of their views, the amblyopia is produced by the squint, in consequence of the habitual negation of the retinal image in the squinting eye, then the question of time is an all-important one, since the success of the operation depends to so great a degree upon the amount of sight retained by the squinting eye at the time of its performance.

The facts which these distinguished authorities have cited, and the arguments which they have advanced, to sustain their views, do not appear to me to be at all convincing, and though the same criticism doubtless applies in a measure to all that can be said in support of the older doctrine, still I cannot but think that the weight of evidence is decidedly in its favor. It has seemed to me, therefore, in view of its practical bearing, that the question which this paper brings up is one which ophthalmologists might discuss with profit.

It is customary to speak of the amblyopia of squint as "*amblyopia exanopsia*," and to place it in the same category with the amblyopia which results, in young persons especially, from the long-continued existence of lenticular or corneal opacity; but this is clearly a mistake, for if the impaired vision be in any sense the result of the squint, it is certainly not due simply to want of exercise of the retina, as is the case with opacity of the media, but to a cerebral process which gives rise to a much more rapid failure of sight, and the outcome of which is better described as a loss of perception in the corresponding cerebral centres, than as an amblyopia of the eye, as Nettleship has suggested.[1] Alfred Gräfe, himself, admits this difference, and yet, realizing, apparently, the unconvincing character of the evidence by which he attempts to sustain his position, he permits himself to use as an argument against the theory that the amblyopia is produced by the squint, the well-known fact that in monocular cataract the exclusion of the retina from participation in vision, even for many years, does not usually (in adult life) result in impairment of its function.

Both Schweigger and Alfred Gräfe[2] reject Müller's theory of congenital retinal identity, and evidently think that by doing so they greatly strengthen their position. I cannot see, however, that such is the case; for, since convergent squint is scarcely ever congenital, and is only rarely seen in early infancy, there is every reason to believe that, whether this fundamental principle of binocular vision—the correspondence of identical retinal points—be an inherited or an acquired law, it is in operation during the pre-squinting years of life; otherwise there would be from the first no stimulus to binocular fixation, and the development of squint in infancy would certainly be the rule, and not the rare exception that it is.

Again, they both refer to the fact that in some cases of non-alternating squint of years' standing the squinting eye retains vision which is nearly, or quite as good as that of the other eye; while in the other cases a high degree of amblyopia is found in squints of very recent development; and this, they think, militates against the view that the squint induces the amblyopia. In reply to this, it may be said that, as a rule, the impairment of vision is certainly greater in squints of long standing than in those of short duration; in the next place, that the non-development of amblyopia in exceptional instances is easily explained without the necessity of abandoning the suppression theory, a marked difference in the refraction of the eyes being competent to produce this result, because the indistinct retinal image which the squinting eye receives under such circumstances causes less confusion, and is, therefore, less apt to be suppressed; and, in the third place, that the development of the amblyopia doubtless begins during the forming stage of the squint, while it is yet unconfirmed, each movement of excessive convergence being accompanied by an effort to get rid of the attendant diplopia, so that it is not at all remarkable that in some instances the impairment of vision should reach a high grade by the time the squint has become fully established; and, furthermore, if this were not the case, who, it may be asked, is in a position to affirm that the alterations of cerebral structure which cause the impairment of vision may not take place within the space of even a few weeks?

Schweigger asks why we usually have diplopia in paralytic squint and not in concomitant squint, and

[1] A paper read before the American Ophthalmological Society, July 22, 1886.

[1] Diseases of the Eye, American edition, p. 238.

[2] See article upon Strabismus, by Alfred Gräfe, in Gräfe and Saemisch's Handbuch, vol. vi., and the chapter upon the same subject in Schweigger's Handbuch der Augenheilkunde.

finds a satisfactory answer in the assumption that in the former retinal identity or coördination has been *acquired;* in the latter it has not been. To my mind, a much more satisfactory answer is, that paralytic squint occurs suddenly, and is usually met with in adults, whereas concomitant squint develops slowly, and almost always commences in childhood. In the former, therefore, we have annoying diplopia, because there has been no incipient stage during which the individual has been learning to suppress the image formed in the squinting eye; in the latter, this faculty, as has been said, is already acquired by the time the squint is fully formed, so that double vision is rarely complained of. Moreover, it is to be remarked that amblyopia from suppression develops more rapidly in children than in adults, and that even where diplopia is present in the early stages of concomitant squint, it is apt to escape our notice, because in children such visual disturbances cause much less annoyance, and are less apt to be complained of, than in adults.

Again, Schweigger urges that the "suppression theory" demands that the image in the squinting eye shall be always and continually suppressed in order to prevent diplopia, but that he has satisfied himself that this is not the case, for he has met with persons in whom it was impossible to produce diplopia, but who, nevertheless, while fixing an object with the good eye in a dark room, saw the image of a candle-flame whenever, by reflection, it was caused to fall upon the yellow spot of the squinting eye. As I understand the suppression theory, it claims that in the beginning there does occur an active exclusion or suppression of the image produced in the squinting eye, but, that in time, in consequence of this process, structural changes take place in the corresponding visual centres, and that, henceforth, we have to do not with an eye which can see, but by an active effort refuses to see, but with one which cannot see though it would; and, furthermore, that when this active process has produced such a degree, and such a kind of amblyopia, as shall do away with the discomforts of double vision, its purpose is accomplished, and it probably ceases to be operative, and as the attainment of this end does not require that there should be loss of light perception in the region of the yellow spot, so great an impairment of vision as this is not to be expected, and, as a matter of fact, is scarcely ever met with. I cannot admit, therefore, that this experiment of Schweigger's has any significance, or that it tends in the smallest degree to disprove the truth of the "suppression theory."

The absence of diplopia in concomitant squint Schweigger explains, as has been mentioned, by assuming that binocular vision (retinal identity) has not been acquired, or, if acquired, has been forgotten; but, as Noyes and others have pointed out, this explanation is not satisfactory, because in many cases in which under ordinary conditions double vision is absent, it can be produced by causing the image of the object looked at to fall upon a different portion of the retina of the squinting eye by means of a prism.

Schweigger makes a seemingly strong point when he states that no one has ever proved that an eye which once saw well has become amblyopic after the occurrence of squint; for, if the "suppression theory" be true, it is exactly this which does happen in most cases of concomitant strabismus. I cannot believe, however, that he has warrant for so sweeping a statement. Possibly he could find no such cases recorded in medical literature, and this is not so remarkable in view of the comparatively small consideration which this subject has received at the hands of modern writers; but that there are scores of cases recorded in the note-books of ophthalmic surgeons in which there has been observed a progressive decline in the vision of squinting eyes, I do not for a moment doubt. Roosa, in his recent paper upon "The results of the operation for convergent squint,"[1] reports one case, at least, of this character. It is to be borne in mind, moreover, that it is always extremely difficult, and often impossible, to ascertain with any degree of accuracy the acuity of vision of very young children, and that when the eye to be tested is a partially amblyopic and squinting one, the difficulties of the task are immensely increased. It is not to be wondered at, then, since the majority of concomitant squints develop, probably, before the sixth year of life, that there should be a paucity of evidence as to the relative acuity of vision of eyes before and after they have learned to squint.

Schweigger's statement that only thirty per cent. of squinting eyes exhibit amblyopia is, I think, scarcely in accordance with the facts; certainly my own experience does not confirm this.[2]

If we turn now to the other aspect of the question, and ask what evidence there is in favor of the view that the amblyopia is produced by the squint, we shall not find the "suppression theory" so devoid of sustaining testimony as Gräfe and Schweigger would have us believe.

In the first place, the character of the amblyopia which we find in squinting eyes is significant. If we ask ourselves what must be the chief sources of annoyance to an individual who has just begun to squint, after having previously enjoyed good binocular vision, we shall find that he has two especial difficulties to contend with—one, the doubling of every object upon which he fixes his attention, in consequence of the false position of the retinal image of this object in the squinting eye; the other, the confusion of vision which results from the images of *different* objects falling upon *corresponding* retinal points, especially in the region of the yellow spots. To get rid of these annoyances he has a twofold task to accomplish—to eliminate the diplopia, he must teach that portion of the retina of the squinting eye that habitually receives the false image of the object which he looks at not to take cognizance of this image; to prevent the image of some object which he is not regarding from being (mentally) superposed and fused with that of the object which

[1] MEDICAL NEWS, Feb. 6, 1886, vol. xlviii., No. 6, pp. 141 et seq.

[2] The sight of the squinting eye is below that of the other in about eighty per cent. of the cases (in which the vision is given) reported by Dr. Roosa in the paper referred to above.

he is regarding, he must ignore all images formed upon the yellow spot of the squinting eye. The successful accomplishment of this task will result in the production of an amblyopia in the squinting eye, which will be regional in character, being most marked in the neighborhood of the macula and over that portion of the retina which receives the false image (so called) of objects regarded by the other eye, and least developed, or altogether absent, in the peripheric parts of the retina which are not concerned in the common visual field. Now, as a matter of fact, does the amblyopia which we find in squinting eyes exhibit these peculiar characteristics, for, if it does, it affords the strongest possible evidence, of itself almost conclusive, that it has been produced by the squint, and that it is not a congenital defect, since such characteristics would be exactly what the one hypothesis would lead us to expect, whereas they would be utterly inexplicable in accordance with the other. I need scarcely say, what every one knows, that its features are, indeed, just such as I have described, central vision and that which corresponds with the part of the retina upon which the false image falls, being especially bad, while peripheric vision, in the direction in which we should expect it to be best, is so good that, as Schweigger, himself, testifies, it helps materially the sight of the individual by enlarging his general field of vision.[1]

Again, the fact that such a monocular amblyopia as we find in squinting eyes, without structural changes distinguishable with the ophthalmoscope, is almost never met with except in strabismic individuals, indicates decidedly that when it occurs under such circumstances it is produced by the squint; for, if it were a congenital defect of such common occurrence as to be a frequent factor in the production of strabismus, it could hardly happen that we could not meet with it quite often in individuals who do not squint. In testing for glasses, especially, it should not be a rare experience to encounter it. That it is, however, an extremely rare occurrence, I think all will agree. Schweigger, it is true, claims to have met with a large number of such cases, but I cannot help thinking that in many of these complicated errors of refraction, which it had not been practicable to work out, were the real cause of the imperfect vision.

The almost immediate restoration of binocular fixation which we frequently see after successful operations for squint, when the amblyopia has not reached too high a grade, is another circumstance which favors the exclusion theory, or, at least, militates strongly against the theory of non-acquisition of retinal identity and the correlated doctrine of congenital amblyopia; for, assuredly, if there had been a failure to establish retinal identity in the first years of life (supposing, for the sake of the argument, that it is an acquired, and not an inherited law), it would be absurd to believe that in later years it might be acquired within the space of a few days.

The theory of congenital amblyopia has doubtless received more general acceptation than would otherwise have been the case, because there are many who think that it affords a more satisfactory explanation of the development of concomitant squint than had been offered previously. While recognizing that a monocular amblyopia would greatly facilitate the development of a squint, if the other conditions favorable to its formation were present, I cannot agree with those who think that Donders's explanation of the origin of convergent squint is insufficient or unsatisfactory. In my opinion, if there be one fact in ophthalmology well established, it is that concomitant convergent strabismus is "chiefly dependent upon hypermetropia."[1] Because hypermetropia is (almost without exception) the essential factor in the production of this variety of squint, it does not follow, as some would have us believe, that all hypermetropic individuals must necessarily be strabismic. That hypermetropia is a far more common condition than convergent squint, Donders fully recognized, and his explanation of this fact —of why some hypermetropes squint, and many others do not—is, to my mind, entirely satisfactory.[2]

In conclusion, I would suggest that, until more convincing evidence is adduced in favor of the congenital amblyopia doctrine than has yet been brought forward by its advocates, it will be judicious for us, in the clinical management of cases of concomitant strabismus, to act upon the older theory—that the squint is the cause of the amblyopia—and, therefore, to give our patients the benefit of the promptest surgical interference.

[1] Schweigger mentions, as a further proof that the squinting eye is used for vision, that strabismic individuals do not have the same difficulty in estimating distance as monoculars; but I am inclined to question the accuracy of this observation; at least, I cannot, in accordance with any theory of binocular perspective with which I am familiar, comprehend how such could be the case.

RADICAL OPERATION FOR STRANGULATED INGUINAL HERNIA; RECOVERY.

BY MARY A. SMITH, M.D.,
OF BOSTON.

THE literature on this subject, and the various methods employed, have been so fully reviewed in recent numbers of the *British Medical Journal*[1] by Wood, Spanton, Vincent, Jackson, and others, that a repetition here seems hardly necessary.

Since, however, the operation has not yet been fully accepted by the majority of surgeons, it is right that every case operated upon should be fully reported.

The following case is of special interest on account of the complications attending it, prior to the final operation.

CASE.—Ellen S., forty-three years old, Irish, widow. Has had two children. Health generally good. About three years ago first noticed a "small lump" in the right groin, but as it gave her no inconvenience, she thought nothing further of it. Last summer she had several "bilious" attacks, at which times she would notice a temporary tenderness in the right groin.

[1] Compare the paper of Dr. Roosa's referred to above.
[2] Accommodation and Refraction of the Eye, pp. 294 et seq.
[3] British Medical Journal, 1885.

October 15, 1885. After lifting a hod of coal patient began to vomit. The vomiting and retching lasted twenty-four hours, and the lump in the groin grew much larger and became quite painful. The bowels moved slightly the following day, but there had been no movement since, when seen six days later, nor any further vomiting. Thinks she passed flatus daily. Had eaten nothing for a week except two crackers, with a little tea and water. Continued to work as a cook until October 20th, when pain and weakness obliged her to leave her place. She walked about a mile to a friend's house, and the next day applied at the New England Hospital Dispensary for relief.

Patient looked pale and hollow-eyed, but there was no marked expression of anxiety. Abdomen was somewhat distended, but not sensitive to pressure, except in right inguinal region, where there was a tumor measuring sixteen by twelve centimetres. The tumor was parallel to and just above Poupart's ligament, elevated above the surface and of hard consistence, giving no impulse on coughing and no fluctuation. The skin was red, hot, and œdematous. Patient was sent home and told to go at once to bed. When seen at 4 P.M., the temperature was 103° F., pulse 120, skin clammy. Tumor was less tense, and an impulse on coughing, and gurgling were now quite perceptible. Two enemata brought away a very moderate amount of scybala and considerable flatus. On account of the local inflammation no taxis was employed, but patient was ordered to remain in the recumbent position and was given only scanty liquid diet. An ice bag was applied to the hernia, and a suppository of opium, 0.06 grm. given.

22d. Morning temperature 101°. Pulse 84. Had passed a restless night. Ice bag had been comfortable to her, and tumor was softer, skin less red. During the succeeding week there was a steady improvement in all the symptoms. The daily large enemata usually brought away a small amount of fecal matter, flatus passed freely, temperature fell to 99.8°, pulse to 76. The signs of local inflammation subsided, and the tumor measured eight and one-half by ten and one-half centimetres. The patient's strength was chiefly sustained by means of rectal alimentation.

On October 29th, without any apparent cause, the tumor suddenly increased in size and became painful, the temperature rose to 103°, pulse to 100, and there was some nausea. A large enema of oil, followed by one of water, brought away only a few scybala. On the following day the temperature was 103.8°; pulse 120, irregular. The hernia now measured nineteen by ten centimetres, and was again quite sensitive to touch, and the skin reddened.

31st. Dr. A. T. Cabot saw the patient in consultation, and agreed to the administration of repeated small doses of castor oil. As signs of obscure fluctuation were now present in the tumor, he advised the use of poultices, to be followed as soon as possible by a free incision. An enema now brought away a large dejection, containing the seeds of some grapes which had been eaten three weeks before. The general condition remained unchanged until the following day when it was time to open the abscess. An incision about seven centimetres long was made from a point corresponding to the external ring and extending outward over the tumor, giving exit to about six ounces of fetid pus and some gas. The hernial sac was found highly injected and adherent to the surrounding tissues. A minute perforation allowed the escape of a small amount of thin feces. Irrigation with a warm solution of potassium permanganate was continued until the abscess was thoroughly cleansed, the cavity was then packed with iodoform gauze and covered with salicylated cotton and a bandage. No attempt was made to deal with the intestinal perforation, as being so small it would probably close of itself.

Later it became necessary to make a second incision about two centimetres inside of the anterior superior spine of the ilium, as the upper part of the abscess was not sufficiently drained through the lower opening.

The patient's general condition now quickly improved, the temperature returning to normal.

The abscess was daily irrigated and dressed as before, and there was no escape of gas or feces into the wound after the second day.

On November 6th, a dose of castor oil was followed by a large movement of soft but formed feces, and from this time on the bowels were regulated with pulvis glycyrrh. comp. The appetite now returned and she was allowed to have solid food. The wounds soon granulated up to a level with the surrounding surface, and cicatrization progressed rapidly. The hernia remained as a small protrusion between the two cicatrices.

On November 13th, patient was allowed to sit up in bed for the first time. That night she complained of great pain on the right side of the abdomen. Next day the temperature was 101°, pulse 104, respirations 40, and the pain had become very severe. As the hernial protrusion had now disappeared these symptoms were attributed to the breaking up of some slight adhesions consequent upon the somewhat forcible return of the bowel into the abdominal cavity which probably took place as the result of sitting up. Opium and repeated mustard poultices relieved this condition, and in the course of a week, after the application of a properly fitting truss, the patient was allowed to be about, and soon regained flesh and strength.

December 24th, I was again summoned, and found that Mrs. S., after ironing on the 22d, had begun to vomit and noticed a sharp intermittent pain in the groin. Bowels had last moved on that morning. The retching and green vomiting had continued for thirty-six hours but were now less frequent. The hernia was found protruding below the truss, but was not inflamed or specially sensitive. Copious enemata brought away neither flatus nor feces, and as hot poultices and opium did not relieve the symptoms, an operation was decided upon.

On the morning of the 25th, therefore, the patient being etherized and the usual antiseptic precautions being observed, an incision was made over the tumor and the tissues divided until the sac was reached. This was found to be everywhere adherent to the surrounding structures. Much time was neces-

sarily consumed in careful dissection, at the completion of which reposition was found to be still impossible. The sac was now opened and the intestine thoroughly examined, it was highly injected and distended, but glistening, and after incision of the ring it was returned to the abdominal cavity. The sac was now grasped with clamp-forceps and torsion made after the manner of Ball,[1] resistance being marked after three half turns had been made. The pedicle was ligated with carbolized silk, and the remainder of the sac below the ligature cut off. A continuous suture of strong carbolized catgut was then employed to unite the pillars of the ring, and fix the stump. To secure additional firmness, one silver wire suture was passed through the pillars and pedicle, its ends being twisted and cut rather short. The superficial tissues were then carefully united by eight carbolized silk sutures, the silk of the lower cicatrix, which had been dissected out in freeing the sac, being utilized for the insertion of a drainage tube. A strip of iodoform gauze was placed over the line of sutures, and covered in by rubber adhesive plaster, and an antiseptic dressing.

Patient recovered quickly from ether, and had no nausea.

Drainage tube was removed on the third day, and the stitches on the seventh. Union by first intention took place throughout the wound with the exception of a small place where the drainage tube was inserted, and this soon granulated out.

Castor oil was administered on the fourth day, and was followed by a free evacuation.

Convalescence was soon established, the patient sitting up on the fourteenth day.

March 16, 1886. The patient is now working as cook and laundress, and shows no signs of any return of the hernia.

[1] British Medical Journal, Sept. 6, 1884.

MEDICAL PROGRESS.

BISMUTH SALICYLATE. — In view of the satisfactory results obtained by Solger and Vulpian from the use of this salt, and the diverse experience of others, LANGGARD calls attention to the fact that the salt is found commercially in two forms, one of which is medicinally inefficacious. The two salts are the acid salicylate and the subsalicylate of bismuth. The former contains forty-one per cent., the latter twenty-four per cent. of salicylic acid. The basic salt (the subsalicylate) is the useful form, and is prepared by washing the commercial salt with distilled water until the wash water gives no violet reaction with ferric chloride.—*Deutsche med. Wochenschr.*, July 3, 1886.

WINTER INDIGESTION.—In an opening address delivered before the Section of Medicine at the Brighton meeting of the British Medical Association, DR. W. H. BROADBENT spoke as follows on the topic above indicated:

As cold and damp weather sets in, there are many persons who begin to suffer from pain after eating, and flatulence; or these symptoms may not set in till later in the winter, when the cold and short days have reduced the vital powers. Very frequently the connection between the indigestion and the season of the year is not recognized, and the subjects of it simply look upon themselves as liable to dyspepsia, which they associate with certain articles of diet, instead of with the winter, or attribute it to want of exercise and fresh air. As is well known, however, cold, and especially cold with damp, will inhibit digestion, sometimes so completely that a hearty meal, eaten with avidity after a cold drive, will be vomited almost unchanged hours afterward, but this takes place more frequently in a minor degree, sufficiently to give rise to discomfort, and a sense of distention, or the cold will inhibit the hepatic functions, and cause constipation.

Now in all such cases it is not the food which disagrees with the stomach, but the stomach which disagrees with the food; and the appropriate treatment is not levelling down the nourishment to the digestive capacity of the stomach, but the bringing up of the functional energy of the stomach to the requirements of digestion, by extra food of a stimulating character, such as beef-tea, or an egg-flip, between meals, by stimulants at meals, and by tonics. So with regard to winter indigestion; winter is not the time for cutting off food, when it is required in larger amount to neutralize the influence of external cold. What is wanted is protection from the depressing influence of cold, or the means of neutralizing it.

It is quite true that most people eat far too much, and, again, that with regard to the stomach, as well as to all other organs and parts of the body, the principle of functional rest is of primary importance in dealing with disease; and restriction of food, and even temporary starvation is often necessary; but we must distinguish, and not starve those who are suffering from inadequate nourishment, or employ treatment for catarrh, or ulcer, or organic disease, when nothing of the kind is present. —*British Medical Journal*, August 14, 1886.

THE RELATIONSHIP OF BODILY AND MENTAL PAIN. —In the opening address before the Section of Psychology at the August meeting of the British Medical Association, DR. T. S. CLOUSTON presented the following suggestive account of the relation existing between neuralgia and melancholia:

In hypochondriasis we have a disease where there is neither acute bodily nor intense mental pain, but a combination of both in a modified and mixed-up way. No one who has seen a strong intellectual man pass through an attack of hypochondriasis, or of melancholia of the hypochondriacal type, and seen him recover again, can doubt that these were real diseases, caused by disordered energizing of the brain-cortex, however much the symptoms during the attack seemed mere "imaginations," and the strange conduct appeared controllable by the will, if the patient had chosen to exercise it. But there are few mental diseases where a man's normal volition, his feelings, his habits, and his power of working, will be more inhibited than in this. The pains and discomforts, the imaginary disorders of function, and the general sense of organic ill-being of the hypochondriac, I look on as resulting from an equal disturbance of the mental and sensory portions of the brain-cortex; just as in ordinary melancholia there is an unequal disturbance, the mental portions being most

affected. No doubt, the areas that control and specially represent the organic viscera, and their special nerve-ganglia are specially disturbed in function in these cases.

From the prophylactic point of view, the general association of melancholia with sensory disturbances has a special value. Some pain or sensory perversion usually preceding the mental disease, the physician can take early measures of prevention. How many attacks of melancholia could be averted if proper measures were only taken in time!

It appears to Dr. Clouston that physicians in general practice frequently do not realize the significance of slight mental depression, accompanying or following neuralgia or sensory perversions, and that psychiatrists often do not sufficiently think of the sensory symptoms in the study and treatment of melancholia. The borderland between neuralgia and melancholia he looks on as a very slight one indeed. They are both manifestations of one type of the neurotic diathesis. The seat of both is in the brain-cortex, and in the same part of the cortex, though not in the same cells. The actual cortical conditions under which the mental and the bodily pains arise would appear to be the same; therefore, it seems clear that they should not be studied or treated apart from each other.—*British Medical Journal*, August 14, 1886.

ESSENCE OF TURPENTINE AS A STYPTIC.—BODIER states that he has frequently arrested serious epistaxis by the use of tampons dipped in essence of turpentine, introduced simply into the nostrils. The essence is partially vaporized by the body temperature, and in this state reaches the most unaccessible portions of the nasal cavities. He has also arrested, by the internal administration of the same agent, intestinal hemorrhages, and has found it of great service in checking bleeding from the gums. In his experience this drug is a more efficient styptic than the perchloride of iron.—*Journal de Médecine de Paris*, August 1, 1886.

CHLORIDE OF SODIUM IN BRIGHT'S DISEASE.—DR. ALLARD MEMMINGER, of Charleston, S. C., highly lauds this use of the drug in the *New York Med. Journ.*, July 31. He has tried it, so far, in only four cases, but his observations are of value, because it alone was used, to the exclusion of all other drugs. At first he orders ten grain doses of the chloride, contained in gelatine capsules, three times a day, and, if the state of the case allows, by preference one hour after or before meals. He generally reverses each day the order of giving; thus, if one day the capsules are given before meals, the next they are prescribed after. If the patient complains of no nausea, he allows him to go about; but at the slightest intimation of a sick stomach, he orders him immediately to assume the recumbent posture, and there remain for an hour or so, after which this temporary ill-feeling always subsides. The second day of treatment he increases the dose to two capsules three times a day, and every other day he increases by one capsule until the patient is taking five capsules three times a day. About this time the good effects of the treatment will be apparent, not only from the improved subjective and objective symptoms of the patient, but from the improved condition of his urine. Albumen will, of course, at this period still be found in abundance—that is, if the case is at all a grave one; even here, however, if a gravimetric examination be instituted a decided improvement, not so much in the absolute as in the relative decrease in albumen, is noted.

At this juncture he orders the chloride to be diminished in quantity, and he has so far found that after the system has thus been brought fully under its influence, it requires but two capsules, three times a day, to keep up the desired effect. If at this stage of the case there is any decided nausea or disinclination to take the medicine, he stops the treatment, and in the interval gives one or two alterative pills, after which he proceeds again to a resumption of the chloride. Should albumen again increase in the urine, urea and chlorides diminishing, he immediately resorts to large doses, thus bringing the patient once more under the influence of the chloride, after which it is again reduced.

The effects of this treatment are most marked. Headache, œdema, low spirits, general weakness, and anæmia, give way to just a reverse order of things, and the patient, who a few days before was most gloomy and desponding, is now full of life and hope.

Thus has it appeared to him in each of his four cases, and, if he has been led to express views that to many appear extreme, it is because his convictions are based upon clinical observations which, up to this time, he has never had the pleasure of recording with any other form of treatment. He would, therefore, urge a thorough trial of this therapeutical agent by the profession, on the following grounds:

1. It is harmless if properly administered.

2. Its effects are comparatively uniform, provided it is given for a sufficient time. That he has so far used it only in chronic cases of no long standing does not, in his opinion, militate against its beneficial effects; for, even should it not be found a cure for Bright's disease, may it not become an important article in our medical armamentarium — indeed, if only an ameliorator of man's sufferings and a prolonger of his life?

3. It may be employed as an adjunct to all recognized methods of treatment without detriment to the patient.

A RESEARCH ON THE REALITY OF REICHENBACH'S FLAMES.—PROF. WM. H. PICKERING, of the Massachusetts Institute of Technology, has presented the following report to the American Society for Psychical Research, which appears in its *Proceedings* for July, 1886.

In the experiment described below, an electro-magnet was employed, with coils measuring eight inches long by five in diameter, and with the pole pieces about half an inch apart. It was placed in a dark room, the walls, floor, and ceiling of which were painted black, and all the cracks covered with rubber cloth, so that not a ray of light entered. A powerful current was passed through the coils of the magnet from a two-light Brush dynamo. After sitting for half an hour in absolute darkness, a faint greenish light was seen in front of the observer's eyes. But it was found that this was quite independent of the magnet, and that it did not disappear when a screen was placed between the magnet and his eyes. When the current through the coils was made or broken by an assistant, no perceptible effect was produced. A person totally unacquainted with physiological optics might readily mistake the subjective phenomenon for an objective reality; but the simplest tests, properly

applied, would distinguish between the two. There was a very powerful permanent horseshoe magnet in the dark room, but no glow could be seen in connection with it.

In the above described experiment, as I was the only percipient tested, it should be mentioned that my eyesight is unusually good in the dark, as is shown by the fact that I can see eleven Pleiades with the naked eye, where many people see only six. If the flames, therefore, were visible to all persons with more or less distinctness, and were not exclusively a nervous phenomenon, I certainly should have made a good subject. As, however, no flames were seen, and the apparatus employed was fully as powerful, if not more so, than that used by the English society, the experiment seems to confirm the view that, even under the most favorable conditions, the flames, if they exist at all, produce no effect whatever on the senses of a normally constituted person.

THE PATHOLOGICAL HISTOLOGY OF COMPENSATORY HYPERTROPHY OF THE KIDNEYS.—LORENZ has made direct micrometric measurements of the histological elements of the kidneys in seven cases of experimentally induced compensatory renal hypertrophy, with the following results:

The hypertrophy consists chiefly in an increase of the cortical substance of the kidney, and, to a much less degree, of that of the parenchyma. The cortical increase is caused in young animals by both hypertrophy and hyperplasia (numerical hypertrophy), while in older subjects the increase in size is due to simple hypertrophy of the elements.

The convoluted tubules were always enlarged, and the epithelium is thicker and wider than in normal subjects. The relatively slight increase of the parenchyma is conditioned by enlargement of the lumina of the straight tubules without increase in the size of the lung epithelium. No hypertrophy of the connective tissue or capillaries was found.—*Centralbl. f. d. klin. Med.*, July 31, 1886.

THE TREATMENT OF RABIES WITH HOANG-NAN.—According to the *Gazette Médicale de Nantes*, twenty-four cases of rabies have been treated with hoang-nan by DR. BARTHELMY and several other medical men of that city or of the department. The first case so treated was in the month of March, 1882, the last in April, 1885. Ten times, at least, the bites, which were most frequently multiple, were situated on the hands, once on the hands and on the face. In the majority of cases cauterization was completely omitted, or practised several hours or even several days after, with not very active agents, such as liquid ammonia, or a solution of carbolic acid. Two of the patients, who were closely observed by Dr. Barthelmy—viz., a man of thirty and a lad of sixteen—presented symptoms of rabic mania: persistent insomnia, anxiety, nocturnal agitation, the desire to run, hallucinations, barking, etc. However, none of these persons felt hydrophobia, nor have any of them, to this date, succumbed. The duration of this preventive treatment was, on an average, twelve days. The total dose of the powder of hoang-nan ingested during this time varied in adults from six to eight grammes. It was scarcely necessary to go beyond one gramme per day to obtain the physiological effects of the medicine—exaggeration of the reflexes, cramps, rigidity, slight trismus. The maximum dose was arrived at progressively, and in some cases the treatment was terminated by gradually decreasing doses. From the above cases the author deduces either that rabies is communicated much more rarely to the human species than is generally admitted, or that the hoang-nan, administered progressively to the physiological effects *during the period of incubation*, sufficiently and efficaciously modifies the nervous system and the entire economy to prevent the evolution of the rabic virus.

SAFETY OF COCAINE IN CATARACT EXTRACTIONS.—DR. G. E. FROTHINGHAM presents, in the *Journal of the American Medical Association* for August 21, 1886, an account of thirty-nine cases of cataract extraction in which cocaine was employed, and deduces from his experience the following conclusions:

1. Cocaine relieves the operator from the embarrassments during the operation for cataract that arise from vomiting, also from the agitation of his patient which results from excessive bronchial secretion or stertorous breathing. These are often very troublesome when ether or chloroform is used.

2. The danger to the result which often arises from nausea and vomiting after the extraction, when other anæsthetics are employed, is very surely avoided when cocaine is selected as the anæsthetic agent and is properly used.

3. The danger arising from the depressing effect of cocaine upon the nutrition of the cornea is no greater than in cases where ether or chloroform is used. The depression of the circulation which often arises from either of them, may affect very injuriously the corneal nutrition.

4. The disturbance of the circulation of the interior of the eye, and consequent danger of panophthalmitis from this cause, is probably less in using cocaine for this operation than in resorting to general anæsthesia.

5. The danger of sepsis and consequent panophthalmitis from the use of cocaine may be avoided by using only fresh solutions.

MEASUREMENT OF LOWER LIMBS.—Prof. C. L. Ford, of the L. I. College Hospital, called attention, in 1862, to the fact that the lower limbs were not always of the same length in the human subject. This statement induced anatomists and surgeons to make an extended series of measurements, and the results confirmed the opinion expressed by Professor Ford. GARSON, in the *Journal of Anatomy and Physiology*, sums up these observations. In seventy skeletons examined, he found the lower limbs equal in but seven. His measurements show that in 54.3 per cent. the left limb was longer than the right; in 58.5 per cent. the left thigh-bone was longer than the right. The right tibia was longer than the left in 41.4 per cent., and the two bones were found equal in but 10 per cent. The difference in the length of the lower limbs varies from one-eighth of an inch to one inch and five-eighths, without any deformity being recognizable. In a series of measurements of the collar bones, only six, in twenty-two cases examined, were found to be equal.

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SATURDAY, SEPTEMBER 4, 1886.

THE MALARIAL GERM OF LAVERAN.

NEARLY six years ago LAVERAN, a French Army Surgeon, announced to the Paris Academy of Medicine that he had discovered, in the blood of patients affected with malaria, an organism which was possibly the germ of the disease. His observations, made chiefly in Algeria, have been extensive, embracing many hundreds of cases, and the full results of his studies have been given to the profession in a work entitled *Traitè des Fièvres Palustres*.

At the time of the publication of his early papers, the attention of those interested in malaria was directed to the bacillus malariæ of Klebs and Tommasi-Crudelli, an *ignis fatuus*, which led many astray; and, although Richard in 1882 reported observations confirmatory of those of Laveran, it has only been within the past six months that the papers of MARCHIAFAVA and CELLI and of TOMMASI-CRUDELLI have aroused a fresh interest on the subject. In this country COUNCILMAN has given the question careful study, and, in a paper read at a recent meeting of the Association of American Physicians, states that he has been able to verify the observations of Laveran. STERNBERG was shown the organisms by Celli, in Italy, and has also had an opportunity of seeing them in the blood of a patient in Baltimore. We have been able, without any difficulty, to find these bodies in cases of acute malaria, and the fact of their presence in the blood in this state can be readily demonstrated. The questions of their true nature, and of their relation to the disease, remain under consideration.

The parasite presents several different forms, corresponding possibly to stages in its development. It occurs most frequently within the red corpuscle, but is also met with in the plasma. Inside of the corpuscle the organism presents the appearance of a rounded or irregular-shaped body, either hyaline or else deeply pigmented. The latter form is most abundant, and the dark granules inside the corpuscles serve as indicators or guides for their detection. On watching these bodies remarkable alterations in outline are seen, which seem unquestionably to be vital—*i. e.*, amœboid. All observers lay stress upon this phenomenon as a distinctive feature. The pigment granules also display Brownian movements.

Free in the plasma small amœboid bodies occur, similar, apparently, to those within the corpuscles. Another phase of existence is presented by a ciliated organism, which appears to be the pigmented body above described, provided with vibratile lashes. This form is much less common, having been found in only 92 of 432 cases by Laveran, and in 4 of 24 cases by Marchiafava and Celli. We have also seen a body of this kind which conformed to the description given by the French and Italian observers. In dry preparations of the blood, the bodies in the interior of the corpuscles stain readily with the aniline dyes.

Granting the truth of these observations, we have evidently here an organism of a character very different from that of those associated with the specific fevers. The description is that of a flagellate infusorian, with an amœboid phase of development, and should this body turn out to be the veritable germ of malaria, it will be the first instance of a widespread endemic affection associated with protozoan forms.

Marchiafava and Celli have proposed for this organism the name *plasmodium malaria*.

We hesitate, however, to accept, without further evidence, its etiological connection with the disease. While apparently a constant feature in the blood of acute cases, they are sometimes in such scanty numbers that it may require several hours' searching to find them. On the other hand, Councilman has shown that in pernicious cases the blood literally swarms with these bodies. Marchiafava and Celli claim to have produced, in five cases, typical attacks of ague by the injection of blood of a malarial patient, and they have found the pigmented bodies in the subjects of the experiment. Cultures have failed, and, until we have more positive knowledge of the life history of these organisms, we cannot feel sure that we have here the true germ of the disease. Von Sehlen has published in Virchow's *Archiv* for May, 1886, an elaborate criticism of these views, and holds that the evidence of the dependence of malaria upon a micrococcus, which he had described, is a much more likely view. He doubts the parasitic nature of the pigmented bodies, and thinks

they result from a transformation of the hæmoglobin under the influence of the micrococci.

In view of the lamentable mistakes which have been made in this disease by so many good observers, the most rigid scrutiny should be exercised in accepting evidence, and it is to be hoped that those who have the opportunities and the necessary technical skill will soon place us in a position to give an opinion upon the *plasmodium malaria* as positive, one way or the other, as we now can upon the *bacillus malariæ*.

THE PASTEUR METHOD OF TREATING HYDROPHOBIA.

IN THE MEDICAL NEWS for June 12, 1886, we called attention to the opinions of FRISCH, of Vienna, in regard to the method of Pasteur for the prevention of the outbreak of hydrophobia. We thought it important to do this because these opinions had been entirely misunderstood, or misrepresented, by a certain number of medical journals, which reported Frisch as strongly endorsing Pasteur's method. Very recently we have noticed a repetition of this misunderstanding of what Frisch has really said, and believes, and, curiously enough, it appears at the very time that Frisch is making two new communications upon the subject, which are to be found in the *Wiener med. Presse*, of August 1 and 8, 1886. In the first, Frisch refers to the previous communication, in which he pointed out the want of analogy between the experiments of Pasteur and his application of preventive inoculations to human beings. He also recalls his suggestion that the true test of Pasteur's theories would be to inoculate a number of animals with his specific virus by trephining, and then to apply his protective method. This suggestion was made in the greatest friendliness, on the 29th of April, but we have not as yet heard that it has been adopted by Pasteur. Frisch, however, has not waited for Pasteur to apply this test to his theory. He has applied it himself. The result is what would have been anticipated by those who have regarded Pasteur's theory as illogical and erroneous.

Frisch inoculated sixteen rabbits by trephining, with a virus made by rubbing up in sterilized *bouillon* a piece of the spinal cord of a rabbit dying after an incubation of sixteen days, after inoculation with virus derived from a rabid dog, and conveyed through two generations of rabbits. This is a typical virus, according to Pasteur. In fifteen of these rabbits Frisch made preventive inoculations with what Pasteur calls the "fixed virus"—the spinal cord of rabbits dying after an incubation of seven days, after inoculation with virus derived from rabbits which Pasteur himself had inoculated and given to Frisch. In the case of the first rabbit, the preventive inoculations were begun twenty-four hours after the trephining, and, in the case of each successive animal, after an additional interval of twenty-four hours more. This was done in order to determine how long before the outbreak of the expected rabies preventive inoculation would be efficient. One rabbit was not subjected to the preventive inoculation, and served as a control experiment. This animal fell sick on the 18th day, and died on the 21st day after being trephined. Of the animals which were subjected to the preventive inoculations, every one, likewise, died with the customary symptoms of Pasteur's rabies after the usual period of incubation. All but two presented the symptoms of rabies between the 13th and the 19th day, and died between the 14th and the 21st day. Two lived to the 28th and 33d day. The thirteenth, fourteenth, and fifteenth rabbits presented symptoms of rabies before the preventive inoculations were begun. In a second series of sixteen experiments, Frisch attempted to shorten the time required for the eleven preventive inoculations of Pasteur by omitting some of them, so as to render the animals sooner fit for the stronger virus. All but one of these animals died, like those of the first series, of rabies. The escape of this single animal out of thirty-two experimented upon, Frisch thinks, is to be accounted for on the supposition that, in its case, the virus with which it was inoculated did not "take."

To test the specific character of the disease of which the thirty-one died, Frisch inoculated portions of the spinal cord taken from them upon other rabbits, all of which died, with the usual symptoms, between the 9th and 14th days. According to Pasteur's theory, this amounts to a demonstration.

Frisch next tested the preventive inoculations on dogs. He inoculated five dogs with virus derived from a rabid dog, and carried through four generations of cultivation in rabbits, the last of which died after a period of incubation of fourteen days. Three of these dogs were subjected to preventive inoculations, two were not. The latter became rabid on the 12th and 13th days; the first died in thirty-six hours, the second was killed in forty-eight hours. Of the three protected dogs, two died of pronounced furious rabies on the 15th and 18th day after inoculation. The third dog was apparently well on the 23d day after inoculation.

Frisch also inoculated six rabbits, by hypodermatic injection, with portions of the cervical spinal cord of a dog with ordinary rabies—what Pasteur calls "rage des rues." Three of these rabbits he subjected to the preventive inoculations, three were left unprotected, and all alike remained perfectly well when he made his report, four weeks after their inoculation.

The conclusion Frisch arrives at from these experi-

ments, is that neither in rabbits nor in dogs is it possible to prevent the outbreak of rabies by means of Pasteur's preventive inoculations, if infection has taken place. The peculiar force of Frisch's experiments lies in the fact that they were undertaken for the express purpose of testing Pasteur's theory by one who is his friend, and who had every reason to hope for a different result from that which he obtained.

It is a curious coincidence, that the President of the Local Government Board stated, in Parliament, on August 26th, that the Commission appointed by the British Government to examine into Pasteur's method were fully satisfied that his treatment for hydrophobia was effective. Until we have the grounds for this opinion, it will be impossible to estimate properly its value, although it must prove encouraging to those who still hope that Pasteur's method will prove to be trustworthy.

In the discussion of Pasteur's recent announcements in regard to the nature and treatment of rabies and hydrophobia, there has been a good deal of feeling shown by Pasteur's defenders and by his opponents. With this feeling we have nothing to do; but, looking at the matter in as judicial a frame of mind as is possible under the circumstances, we cannot but feel that the true state of the discussion may be still summarized in the expression used in these columns last June, when we stated that there is as yet no good evidence that the application of the theory of Pasteur has rescued a single patient who without his intervention would have had hydrophobia.

DILATATION OF STRICTURE OF THE ŒSOPHAGUS AFTER GASTROSTOMY.

THE advantages to be derived from an operation by which food can be introduced into the stomach notwithstanding the presence of a stricture of the œsophagus are well recognized, but the value of the operation of gastrostomy will be greatly enhanced if it can be made the means of curing the stricture itself. Several surgeons have made attempts in this direction. Bergmann and Schattauer, for example, have succeeded in passing bougies through a stricture of the œsophagus from below upward; and WEINLECHNER has passed a delicate sound down through the stricture, and out of the opening in the stomach. Another method of utilizing the gastric fistula consists in passing a slender sound from above the stricture, and out of the artificial opening, then attaching a thread to its upper end and drawing after it larger instruments. This method has been employed by Weinlechner, who used the thread to draw into the stricture pieces of drainage tube, which were allowed to remain in position, in the hope that their elasticity would have the effect of steadily dilating the stricture. In the only case in which he did this the patient died of tuberculosis. A similar procedure is described by VON HACKER in a monograph on operations upon the stomach, published this year. His plan was to attach a drainage tube to a slender sound, and draw it through the stricture in a state of extension. He believed that when the tube was no longer stretched it would regain its previous calibre, and produce active dilatation of the stricture. It does not appear that this plan has ever been put into practice; and it is easy to see certain objections to it, as, for example, that the elastic tube might break; that its end might double over at the point at which the thread was tied and injure the diseased part of the œsophagus; that it might, if small, become wrinkled and not expand at all; or, if thick, that it might become intolerable or dangerous to the patient.

An apparently much better method has recently been proposed and used by MADYL, who describes it in the *Allgemeiner Wiener med. Zeitung*, for June 15, 1886, as follows: He introduced through the cicatricial stricture of a patient, twenty-two years old, a fine bougie, No. 5; he then anæsthetized the patient, passed a sound into the opening in the stomach, which he had dilated with a sponge tent, found the end of the bougie, seized it with urethral forceps, and drew it down till it protruded from the stomach. He next pierced the upper end of the bougie, which protruded from the mouth, with an awl and attached to it a strong double silk thread twice as long as the bougie. He now drew down the bougie through the œsophagus and stomach and pulled the thread after it. He then tied the upper end of the thread into the lower end of a large bougie, No. 10, drew this carefully down through the stricture, and left it in place for twenty-four hours. In order to make the treatment as little irritating as possible, Maydl took the precaution to draw the upper end of the bougie down into the œsophagus, after having attached a thread to it, which was secured to a pledget of gauze retained in the nose. He also secured the patient's hands, so that he could not pull the bougie out when it proved irritating. In twenty-four hours the bougie was withdrawn through the stomach and with it the thread was drawn down, and was then left in place for three days. At the end of this time a larger bougie was drawn down by means of the thread, and was left in place for twelve hours. The subsequent steps consisted in introducing a larger bougie every second day, diminishing the time which it was allowed to remain, and leaving the thread in during the intervals. In order to reduce to a minimum the irritation of the manipulations, a small quantity of a five per cent. solution of cocaine was swallowed. Maydl found it best to introduce the thread through the nose, and not by

the mouth, because it is less irritating in the former situation.

The obvious advantages of this ingenious method are that when once the opening of the stricture is found, the way to it is never lost. This is sometimes no small matter, as it is common to find an enlargement of the œsophagus above the seat of a stricture, and a small bougie may easily miss the central channel. The retention of the bougie in the stricture is likely to have a similar effect to that of a *sonde à demeure* in the urethra, and to lead to a more speedy resolution of a stricture than could be hoped for from the repeated passage of such instruments.

The tediousness of the old method of dilatation need not be described here. It is more important to note that in a few days Maydl's patient reached a point where he was able for the first time in a year to eat a piece of meat in the natural way, and that the operator hoped in two weeks to be able to pass through his stricture the largest bougies. This success is encouraging, and on its face the method seems well adapted to the treatment of cicatricial strictures of the œsophagus. Of course, much is not to be expected from it in case of malignant strictures.

SOCIETY PROCEEDINGS.

THE AMERICAN DERMATOLOGICAL ASSOCIATION.

Tenth Annual Session, held at Greenwich, Conn., August 25, 26, and 27, 1886.

WEDNESDAY, AUGUST 25.—FIRST DAY.

MORNING SESSION.

THE PRESIDENT, DR. EDWARD WIGGLESWORTH, of Boston, called the meeting to order, and opened the session with the following

PRESIDENTIAL ADDRESS.

Gentlemen: Just ten years ago, at Philadelphia, I had the honor of calling to order the first public meeting for the organization of this Association. Our specialty for the first time received national acknowledgment. Our guerilla warfare was over, and our subsequent record has been one of unbroken, successive victories over bigotry, error, and ignorance.

But the day of paladins is past and our ranks will admit more recruits. There are still remaining opportunities for more extended instruction of students in medical schools, while the lack of hospital facilities for the proper clinical treatment of diseases of the skin, is a standing shame and disgrace to our municipal authorities, our hospital boards, and to the medical profession itself. "As now arranged nearly everywhere amongst us, these institutions refuse, to a large class of sick persons, the benefits of hospital residence either wholly, or offer them, if received, not, we may fairly say, the best medical skill it is in their power to procure." Some progress has, however, been made. New York has now a special hospital for skin diseases, besides two wards in the Charity Hospital, and Philadelphia has special clinics with beds at three different institutions.

That the workers are ready as soon as they shall have opportunity is evident. Ten years ago our officially recognized representatives were, four professors, five clinical professors, one instructor, and six lecturers; total sixteen. To-day we number seventeen professors, eleven clinical professors, six instructors, and sixteen lecturers; total fifty. Nor do I include genito-urinary surgery, which I regard as a distinct specialty from that of dermatology, which, of course, includes syphilis.

About twice as many cases of diseases of the skin are now annually treated by specialists as there were ten years ago. The contributors to American dermatological literature during the past decade include the best treatises, handbooks, and atlases, the only periodicals, and some of the best monographs of the time, in the English language. Our sole local organization, the New York Dermatological Society, is as active as ever, and the good work which it has already done is constantly increasing.

Two honorary and three active members of our original Association have passed away since its organization. Twelve active members, all living, have been added to its ranks. So much for our material progress and present prosperity.

Other objects of the foundation of our Association have been by no means neglected during the past ten years. There exists to-day a harmony and good feeling amongst us which, but for the personal acquaintanceship resulting from the formation of this Association, never would have existed. We have come into better accord as to the "nature and treatment of skin diseases." Our increased membership proves that our Association is "held as a place of aspiration;" and it has consistently required "independent work in our department" as the essential preliminary step toward the acquisition of such membership. The systematic work of our standing committee upon statistics has already furnished valuble data for comparison, as well as a series of excellent reports upon "leprosy in America." Concerted official effort has done much and will do more to repress the rank luxuriance of growth of dermatological nomenclature and to furnish a "uniform and simple system."

While much has been accomplished, much still remains to be done. The idea of specialism, already rooted, is to be nurtured and trained in the minds of the profession and of the public, and the practical development socially of our specialty itself is to be elaborated in many minor details. The pioneer must no longer prove a martyr. A knowledge of the weaker side of human nature, useful for selfish financial ends, must no longer be allowed to usurp the province of exact scientific acquaintance with disease, inuring to the benefit of our race. The "elaborate division of labor" is "as useful and successful in a learned profession as it is in the mechanical arts," for it is merely a relative question of height of standard. Specialism substitutes quality for quantity, which substitution is the essential characteristic of the civilized man as distinguished from the savage, while the rapidity of such substitution gauges the progress of civilization itself. Medicine is merely that complex whole which results from the combination of all its component parts, and

their individual advancement is the criterion of its own progress.

Science is only exact knowledge. Medicine is that knowledge specially directed to the physical welfare of mankind, and specialism is only that further subdivision rendered necessary by the very various parts composing the individual, and possible by the extension of our opportunities for studying these parts due to increase of mechanical means for enlargement of the fields of our hitherto unaided senses. No one can to-day "take all knowledge to be his province." Science does not culminate in a Jack-at-all-trades, least of all in one whose conscience has become anæsthetized by custom, who confounds his own limitations with the "limitations of human understanding," and his own ignorance with "the immaturity of medical therapeutics." The specialist builds his own boundary wall and cannot, if he would, poach upon the preserves of others. He "distinguishes what he can do from what he cannot," thus filling the old definition of the best physician. He cannot maltreat a patient, and when compelled to confess ignorance and seek superior wisdom, charge the wronged sufferer another and still higher fee for a "consultation," which is in reality a confession. It has been sarcastically called "the sole duty nowadays of the family doctor is to decide what specialist shall be summoned." It certainly is his duty, and a very important one as a man of honor, to decide whether any one, and if so, who can probably accomplish that wherein he admits that he himself has failed. Unfortunately, many know so little that they are even ignorant of how much is known by others; but in many respects the specialist has already raised the average standard of requirement for general practice to such an extent that much of the old routine practice of physicians who treated their patients for the very diseases for which they referred members of their own families to the specialist, has now become punishable malpraxis. But the general practitioner has his revenge in opposing as unnecessary the hospital appointment, and even the private practice of the very specialist to whom he himself flees as "a help in time of trouble."

We are told that "the human body is made up of parts and functions so thoroughly interdependent, that it cannot be parcelled out into defined and isolated regions." It can and it can not. What scientist divides a country into square miles and attempts to become thoroughly conversant with every atom existing in each of those miles? The special divisions of study are rather the flora, the fauna, the geological strata, etc. though all these may pervade identically every mile of the region. If regional surgery is possible, then specialties are certainly not contraindicated. We hear of "appalling pathological conglomerates" due to lack of proper "general medical treatment." Not only "conglomerates," but often single lesions are very variously diagnosticated by different general physicians, and the "conglomerates" are usually merely the aggregated effects of original causes which might have been obviated by proper investigation in due season, on the part of suitable specialists.

The aforetime "consulting physician" meant one who, in addition to his general acquirements, knows more about some particular thing than any one else. We may be sure that his general acquirements had to pay the penalty. To-day, we honestly admit this, and renouncing the practice obtainable by general acquirements, keep to that particular thing of which we know more than others. No one has mental ability enough at the present time to add to the greatly extended knowledge necessary to the general physician, the intensified fundamental acquaintance with detail needed by the specialist. Life is too short for the ablest intelligence to exhaust even any one specialty. The physician may, like Newton, "think the thoughts of God after him," but the thoughts of the infinite upon the smallest molecule of matter, call for more than the limits of a finite existence. The true consulting physician of to-day is the specialist, and he should therefore receive this title at the hospitals with which he is connected, while those ex-physicians, called consulting, but never consulted, should receive their true and proper title of Emeriti.

But enough of the "idea of specialism." The public will, in time, appreciate the absurdity of being content with inferior results in one branch of medicine, because, forsooth, there are so many others in which their medical adviser is equally or more at home. It will reason rather that he who "insists upon doing the work of ten men, manifests a quality of mind which we can only call arrogance, and which challenges for his work severe criticism." Versatility will not atone for crude and imperfect work, any more than will lack of time, hurry of life, keenness of competition, or financial necessity, and it is merely brazen self-assertion which delights "not so much in doing the thing well as in showing how well he can do it."

Believing in the ultimate achievement of all possibilities and in the progress of truth, I have no doubt as to the future of specialism if we are honest and earnest. Not infinite omniscience, but infinite morality is the duty of the specialist, and this conscientiously carried out, will blunt the sharpest dart of the hostile general practitioner. While visions of what yet remains to be accomplished might well lead us almost to despair, a mere glance at the generally prevailing ignorance on the part of both physicians and their patients, shows conclusively that we and our work are imperatively demanded.

Dr. Wigglesworth next considered in detail the duties, in the future, of members of the Association as to the establishment of special skin wards in all general hospitals; as to a revision of the existing nomenclature and classification of skin diseases; as to improvements in the annual volume of *Transactions;* the use of the metric or international decimal system; more extended statistics; the establishment of a permanent committee upon new remedies; the question of fees and the remuneration of physicians by others of the same profession; the desirability of a (proposed) "consistent schematic plan for the report of cases," always accompanied by a summary; and the duties of specialists as to instruction. He mentioned the establishment in New York of a society for the prevention of contagious diseases. Attention was also called to the proposed union of all the special medical associations of America into a general congress.

DR. E. B. BRONSON, of New York, presented an essay

ON ERYTHANTHEMA SYPHILITICUM.

He called attention to certain cutaneous eruptions

of an erythematoid character occurring in connection with syphilis, and desired so far as possible to determine their etiology. A case was reported of a syphilitic patient who developed a peculiar eruption upon the head, hands, and feet. Although the eruption presented no pronounced syphilitic characteristics, it was regarded as due to syphilis, for the reasons that it was shortly followed by an outbreak of well-marked specific efflorescences on various parts of the body, and, moreover, there was a transformation of the eruption upon the head, into infiltrated lesions presenting a distinct syphilitic character. The eruption first appeared on the face in the form of an erythema which was covered with vesiculo-papular efflorescences. Later a similar outbreak occurred on the nape of the neck. The vesiculo-papules broke, leaving excoriated surfaces of a diphtheroid character, from which there exuded an exceedingly abundant, foul-smelling discharge. Soon after, vegetating growths made their appearance, which upon the bearded parts of the face were quite exuberant and resembled condylomata acuminata. Those upon the neck, formed more discrete, tuberous prominences and had the appearance of condylomata lata; while upon the upper parts of the face, the elevations were nummular and had comparatively smooth, though somewhat lobulated surfaces. After a week or two, the erythematous areas, which formed the bases of the vegetating lesions, became the seats of a gradually increasing, copper-colored infiltration. The palms and corresponding surfaces of the fingers, were covered with a deep red, sharply circumscribed erythema, which was followed by lamellar desquamation. A similar eruption occurred in patches upon the soles. The finger-nails were all more or less affected with onychia sicca. Upon the body and limbs several crops of well-marked syphilitic papules succeeded each other, and finally all the manifestations yielded rapidly to mercurial treatment. The preliminary eruption in this case was characterized as an erythanthema dependent in some way upon the specific disease. Other instances of erythematoid eruptions occurring incidentally to syphilis, were referred to as being more or less analogous to the above cited case.

Allusion was made to syphilitic roseola as a possible case in point; then to the preliminary erythematous eruptions observed in hereditary syphilis, *érythème précurseur* of the French, and also to certain forms of coincident or preliminary erythema as described by Hebra, Neumann, Danielssen, Lipp, and Finger. Attention was next called to that class of symptoms which have been characterized as syphilitic erythema nodosum, and three cases of this affection were reported, in one of which the erythematous eruption was apparently succeeded by a syphilitic infiltration which pursued a course similar to that of a breaking-down gumma of the skin.

Finally it was attempted to define more precisely the etiological relations of these various erythematoid eruptions occurring in connection with, and probably as a consequence of, syphilis. It was maintained that probably they were not equivalent in their mode of origin to the syphilodermata, but rather to certain angioneurotic affections, which they closely resembled. The true syphilodermata are always characterized by a specific infiltration and are probably the result of a determination of the materies morbi to the surface. They are, therefore, pathognomonic of syphilis. The erythanthema syphiliticum, on the other hand, is only an indirect effect of the syphilis, the result of reflex irritation of the skin from the action of the disease upon nervous centres, and therefore not pathognomonic of syphilis. The fact that this might be followed by a true syphilitic infiltration *in situ*, was regarded as analogous to the effect sometimes observed of traumatic irritation in syphilitic subjects, where local injuries become the seat of specific infiltrations.

DR. I. E. ATKINSON, of Baltimore, said that as regards pathognomic syphilitic eruptions, a great deal of caution must be exercised. Lesions similar to those described by Dr. Bronson are not of excessive rarity in cachectic subjects and in cases of malignant syphilis, but in his experience, they have always been manifestations of late or tertiary syphilis.

DR. J. C. WHITE, of Boston, remarked that the occurrence of the forms of disease which have been described are so rare in syphilis, we know so little of the nature of erythema nodosum and its causes, and so little of the nature and causes of so-called erythema herpetiformis, which he thinks should be called erythema multiformis, that it seems to him that we must have some more exact data before we can regard these affections as a necessary sequel of syphilis and as anything more than accidental occurrences. A causal relation existing between these conditions and syphilis has not been proven. The early roseolous exanthematous condition of the skin in syphilis is of sufficiently frequent occurrence to be looked upon as a distinct feature of syphilis. This is wholly unlike the rare forms of dermatitis which have just been described.

DR. F. B. GREENOUGH, of Boston, stated that syphilitic roseola seemed to him to be decidedly distinct from other forms of congestion of the skin, especially in one particular, that is during the subsidence of the congestion little points of distinct congestion continue for a long time. These appear to represent the follicles. The points of pigmentation are also an almost constant occurrence. This he had not seen in other forms of affection of the skin.

DR. G. H. TILDEN, of Boston, held, in the present state of our knowledge, that it would be more philosophical to consider the appearance described as accidental than as dependent upon the specific disease.

DR. I. E. ATKINSON, of Baltimore, then read a paper on

RÖTHELN,

which was based upon a review of the literature of the subject, and a study of the reported cases. The disease was regarded as distinct from measles and scarlet fever. The period of incubation varies between two and three weeks. In many cases there is no prodromal stage. There may, however, be prodromata for twelve or twenty-four hours. Catarrhal symptoms are generally absent, but they may be present. Sometimes there is congestion of the throat. Swelling of the lymphatic glands is a peculiar and marked symptom. This swelling is painful, but does not pass on to suppuration. In many cases fever is absent. The eruption first appears on the forehead and face, and in the course of a few hours spreads to other parts of the body. The eruption is more or less irregular, being more intense in some

situations than in others. From the beginning to the disappearance of the eruption is usually three or four days. In the greater number of cases, desquamation fails to occur. Complications and sequela are not unknown, the most common being bronchitis, pneumonia, and gastro-intestinal disorders. Renal affections may follow this disease. Relapses occasionally occur, but not later than the end of the second week. The prognosis is almost always favorable.

The only difficulty in diagnosis is in connection with measles; and in isolated cases a thorough consideration of all the points is essential to a correct conclusion. Very little treatment is required, except in the presence of complications.

On account of the confusion existing as to the nomenclature of this disease, he suggested that rubeola be accepted as its proper title, and that it might be known in popular language as epidemic roseola.

THE PRESIDENT stated that several years ago he suggested that measles be called morbilli, and rubeola or rötheln be used to signify what is termed German measles, and roseola be reserved as the title for the rose-colored spots seen in syphilis and other conditions. There did not seem to be any necessity for the introduction of a new term.

DR. G. H. FOX, of New York, then presented a

REPORT OF A CASE OF LYMPHADENOMA (MYCOSIS FUNGOIDE), AND AUTOPSY.

Mrs. G., aged thirty-three years, the mother of six children. During pregnancy, in the summer of 1881, she suffered with general pruritus, which passed away after confinement in October. A year later, small, flattened, circular tumors appeared in the axillæ and on the breasts. These became moist, and were accompanied with a burning sensation. The eruption disappeared from these situations, and reappeared upon the back and other portions of the body. After the birth of her last child, in February, 1885, a tumor developed at the inferior angle of the left scapula. The lesions on the other parts of the body disappeared, with the exception of two spots on the finger and one on the left cheek. The tumor on the left scapula gradually softened and disappeared.

The patient was admitted to the hospital in October, 1885, and was fairly well nourished. There were at this time numerous tumors over the body, some of which were superficially ulcerated. The worst tumors were on the breast. Hypodermatic injections of Fowler's solution were used without benefit. Chaulmoogra oil was given in increasing doses until forty drops, four times daily, were taken without causing any improvement. The patient died in April, 1886. The microscopical examination gave the usual appearances of these growths. The internal organs were found to be normal.

DR. G. H. TILDEN, of Boston, remarked that, one year ago, he reported a case of this disease. He then said that it was not sarcomatous. Rindfleisch states that he has found a distinct micrococcus inside of the bloodvessels in this connection, and recently this has been confirmed by another observer. It looks as though what we ordinarily know as malignant lymphoma, affecting the lymphatic glands, with deposits in other parts of the body (Hodgkin's disease), is one of the infectious diseases. He should not be surprised to find that this mycosis fungoide is a variety of the same disease, and that it is nothing but an acute infectious disease.

A NOTE RELATIVE TO THE BULLOUS ERUPTION OCCURRING AFTER INGESTION OF IODINE COMPOUNDS

was, in the absence of the author, DR. J. N. HYDE, of Chicago, then read by the Secretary.

Case I.—An infant, seven months of age. Six weeks before coming under observation the child had suffered from a febrile attack, the exact nature of which was not known. Three weeks later the eruption made its appearance. The lesions at first presented the appearance of chicken-pox. The child's face, the dorsal aspect of the hands, forearms, and the scalp, were covered with isolated whitish, split-pea, to marble-sized, flattened, slightly umbilicated lesions, resembling molluscous tumors. The contents of the lesions were inspissated, thick, and grumous, and of such consistency that they did not escape on rupture of the sac. It was learned that iodide of potassium in one grain doses had been administered for the febrile attack. On the discontinuance of the iodide and the use of antiseptic powder, the lesions rapidly disappeared.

Case II.—A lad, fifteen years of age, who had recently undergone an operation for necrosis of the right femur, developed an eruption involving the entire body. There were two distinct sets of lesions. That which predominated was composed of annular circinate plaques, varying in diameter from several centimetres to half a metre. There were also here and there, particularly about the finger-ends, distinct blebs. The patient had been taking iodide of potassium in fifteen grain doses. This was discontinued, and the eruption speedily disappeared.

There are two distinct types, the bullous and the quasi-bullous eruption, produced by the ingestion of iodide of potassium. In the first form the patients are often well advanced in years, often cachectic, and frequently affected with syphilis or other grave disease. In what may be regarded as a second sub-form of this exanthem, there are dimorphic or polymorphic symptoms. Here bullæ are commingled with papules, tubercles, and other patches of disease. The speaker then described other cases of similar eruptions described by various writers, and, in conclusion, presented the following interrogative propositions:

1. Are there not three sub-forms of the bullous exanthema developed in certain individuals after the ingestion of iodide of potassium?

2. Is not the first and most common of these to be generally recognized in persons of advanced age and cachectic condition, the rash being then present in the form of typical bullæ?

3. Is there a second and still rarer sub-form, in which the eruption is displayed in dimorphic or polymorphic manifestations; typical, perfect bullæ being then mingled with papules, tuberoles, scarlatiniform maculations, or with other and different lesions?

5. Is there not a third and rarer sub-form, the quasi-bullous rash, to be most frequently recognized on the face and dorsal aspect of the hands and forearms of infants and children, in which the lesions are semi-solid, slightly umbilicated, and filled with inspissated, yellow-

ish matter, and which may shrivel and desiccate on the suspension of the drug inducing the condition?

5. Is this last described lesion one to be recognized solely as the result of the ingestion of iodide of potassium, never under other circumstances, and one as peculiar to the special condition it represents, as is the gumma to syphilis?

DR. R. W. TAYLOR, of New York, said that while the most general eruption produced by iodide of potassium is an acneform exanthem, yet it may cause dermatitis with multiform lesions with bullæ, which may contain serum, pus, or even blood. He thought that some of the cases of extreme dermatitis multiformis which have been described have been due to this cause.

He described a peculiar form of iodine intoxication in a case of gummatous infiltration of the pharynx, in which iodoform had been resorted to as a local application. In the course of two hours it produced a diffuse eruption of the face, neck, and hands; on the hands it assumed an eczematous character.

DR. J. E. GRAHAM, of Toronto, related the history of a case similar to that described by Dr. Hyde. A man, twenty-two years of age, had contracted syphilis two years previously. Noticing some pimples on his face he secured a preparation of iodide of potassium containing five grains to the dose. The speaker saw him two days after he had begun to use this preparation. He then had a bullous eruption, principally upon the face and neck. At that time it was thought that it was a tertiary manifestation, and he increased the dose of iodide to fifteen grains. Two days later, the eruption on the face was much increased and recognizing the cause of the condition, the remedy was stopped and the eruption rapidly disappeared.

DR. I. E. ATKINSON, of Baltimore, said that he saw a case, one year ago, of what was supposed to be malignant syphilis. The patient had had syphilis, and had been put on the use of iodide of potassium. The lesions continued to increase and after the iodide had been taken for six months he saw the case. There was an enormous formation of scar-tissue over the face and neck. In addition there were bullæ and elevated infiltrated plaques. The appearances presented were unlike those produced by syphilis, and he thought that they were due to the iodide of potassium. The drug was stopped, and in six weeks the ulcerations had healed; the cicatricial tissue, of course, remained, Too much stress cannot be laid upon the dangerous conditions which may follow the administration of the iodides.

EVENING SESSION.

R. W. TAYLOR, M.D., of New York, read a paper on

PRECOCIOUS GUMMATA.

The following conclusions were presented:

1. Like the osseous affections, the affections of the nervous system and malignant syphilides in general, the gummata syphilæ may be precocious in appearance, occurring as early even as the second month of infection.

2. Of the precocious gummatous syphilide or gummata, there are three quite clearly marked forms, first, the early, general, and copious form; second, the more localized form, which may invade several regions, is usually symmetrically distributed and sometimes even is confined to one region, particularly one side of the face or scalp and the roof of the mouth; and thirdly, a form in which more or less severe neuralgias precede and accompany the eruption, which in many particulars resemble simple erythema nodosum, but which in its etiology is not in any way related to this simple form of eruption, but is a direct outcome of the syphilitic diathesis.

3. That these precocious gummata partake in general of the features of those of later forms, but that they differ in the more acute invasion, in a much more rapid course, and are usually not as profound and destructive in their action as the classical eruption.

4. That of these precocious forms of gummata, there are found to be two varieties, one a non-ulcerative or resolutive, the other ulcerative.

5. That in the treatment of these precocious syphilides, a combination of mercury and iodide of potassium is much more efficacious than is mercury alone.

DR. F. B. GREENOUGH, of Boston, presented some

CLINICAL NOTES ON SCABIES.

For thirteen years he had had the opportunity of studying a large number of cases of scabies, and had been much interested by the rapid increase in the number of cases seen during the past few years. They had rapidly increased from 3 in 1879, to 160 during the last year. He had during the past year also made inquiries as to the number of members of the same family of whom the patients could give an account as being similarly affected, and had records of 110 such cases, which, with 6 cases seen in private practice, made a total of 276 cases that had come under his cognizance during the past year. The percentage of cases of scabies had increased from 3 per cent. in 1876 to over 13 per cent. during the past year. During the war, the disease naturally increased and the medical officers got a knowledge of it, which they carried with them into private practice after the war was over. The disease being properly treated became so much less frequent that the new generation of physicians saw little of it, and each case not recognized proved a focus of contagion for a large circle. Four-fifths of the cases seen were between five and thirty years of age, and only four cases were over sixty years of age.

The facts which were especially noticed were the few cases in which typical burrows could be found; the great constancy of the manifestations on the penis in male subjects; and the success of treatment. He had used almost entirely an ointment consisting of two parts of carbonate of sulphur, one of carbonate of potash, and three of petroleum ointment, simply cautioning the patients against applying it to inflamed and pustular localities. He referred to the distribution of the eruption as of importance in the diagnosis of difficult cases.

DR. A. R. ROBINSON, of New York, said that in some cases in which there was difficulty in the diagnosis, he had reached a positive conclusion by opening a vesicle and examining its contents under the microscope, and finding the young acari or the feces.

DR. L. N. DENSLOW, of St. Paul, had seen a number of cases in which no burrows could be found, but under treatment suitable for scabies, they had recovered. He employs thorough scrubbing from head to feet, followed by the application of sulphur ointment. He considered

the complaint known as prairie itch nothing more than scabies.

DR. E. B. BRONSON, of New York, remarked that in many cases he had depended largely upon the elongated character of the efflorescence in making the diagnosis. For three months he had treated all cases of scabies with naphthol, using a fifty per cent. preparation and making one thorough application. This has produced a satisfactory result.

DR. S. SHERWELL, of Brooklyn, had also noted an increased number of cases of scabies, and a large proportion of the cases had been among Scandinavians, leading him to think the increase might be due to want of thoroughness in the cleaning of the ships in which emigrants are brought to this country. As a prophylactic, he directs the patient to sprinkle on the sheet of the bed a teaspoonful of dry sulphur. This acts as a disinfectant and is unpleasant to the acaris.

DR. H. W. STELWAGON, of Philadelphia, presented some

CLINICAL OBSERVATIONS REGARDING THE VALUE OF RESORCIN, ICHTHYOL, AND LANOLIN IN CUTANEOUS DISEASES.

Resorcin, in eczema, is rarely of benefit, but possesses some power over the itching. For this purpose a five per cent. ointment is useful. In greater strength it is irritating. In tinea sycosis it has proven of some value in ten to twenty per cent. strength. In tinea tonsurans it is inferior to the remedies usually employed. In one case of leg ulcer, healing took place under its use. In a second case, no effect was produced. In seborrhœa and alopecia dependent upon this disease, a lotion consisting of a drachm of resorcin, one or two drachms of castor-oil, five minims of Peruvian balsam, and four ounces of alcohol, was of value. In tinea versicolor, it was found less useful than a solution of hypophosphite of sodium. In psoriasis and in one case of lupus erythematosus the result was negative. In one case of superficial epithelioma, a fifty per cent. ointment produced a good result, in a second case a fair result, and in a third the result was negative. In a case of favus, a twenty-five per cent. ointment, used for two months, produced no decided effect.

Ichthyol. In a small proportion of cases of rosacea and acne vulgaris, a ten to twenty per cent. preparation usually was found eneficial. In eczema it was valueless and irritating. In furunculus it acted with good results in three cases, when applied as a twenty per cent. plaster. In the fourth case it had no effect. It was of service in psoriasis and also in a case of lupus erythematosus. In favus it was used without effect.

Lanolin. In some cases, as an ointment base, this is superior to the ordinary fats in use. Where a simple protective action is desired, it is inferior to vaseline, cold cream, or lard. In chronic cases, where there is infiltration and a degree of penetration is desired, lanolin is especially valuable. The writer stated that, according to Liebreich, a lanolinum purissimum is now manufactured in which the cholesterin ethers are absent. The main disadvantage of lanolin, as now manufactured from sheep's wool, is the strong sheepy odor. In a few acute and subacute cases of eczema, lanolin, for some reason, proved irritating. As a rule, however, it is bland and unirritating.

(To be concluded.)

THE AMERICAN SOCIETY OF MICROSCOPISTS.

Annual Meeting, held at Chautauqua, New York, August 10, 1886.

(Specially reported for THE MEDICAL NEWS.)

A VERY successful meeting of this Society was held this year at Chautauqua, and we present abstracts of the papers read which were of medical interest.

DR. GEORGE W. LEWIS, JR., of Buffalo, presented

A REPLY TO SOME OF THE ARGUMENTS THAT HAVE BEEN ADVANCED TO OVERTHROW THE COMMA-BACILLUS OF ASIATIC CHOLERA.

On the 25th of July, 1884, Dr. Koch addressed the Imperial German Board of Health at Berlin on the subject of cholera and its bacillus. He then expressed the belief that the comma-bacillus bears a causal relation to the cholera process: that it gains entrance by the mouth, and depends for a safe passage through the stomach upon some disordered condition of that organ or its function; that the real seat of the disease is the lower part of the ileum, including Peyer's glands; and that up to that time inoculation experiments had given only negative results. He pointed out, moreover, a striking relationship between the course of the disease and the life history of the organism both as regards the remarkably short time required for it to reach a full development, and the equally rapid decline which immediately sets in. His statements were the more valuable because they were based upon some three hundred autopsies performed in the height of cholera epidemics in France, Egypt, and India, and because, not satisfied with even this, he had visited the long stretch of country known as the Delta of the Ganges, where the disease prevails to a greater or less extent throughout the entire year.

From the date of that memorable meeting to the present time, the comma-bacillus has been a favored target at the hands of the skilled and unskilled in this branch of medical research. To attempt an answer to some of the so-called arguments that have been advanced to overthrow the comma-bacillus and its relation to the cholera process would be a mere waste of time, for the promulgators of these theories show by their methods of reasoning their utter ignorance of even the principles upon which the science of bacteriology is based. Such, for example, are those who claim a spontaneous origin for this disease, and likewise those who assert, apparently without any foundation, that the comma-bacillus is only a transition stage in the development of the organism which really causes the pathogenic changes. This idea, he believed, grew out of the fact that inoculation experiments are not altogether positive in their results, although it is generally conceded that the comma-bacillus is in some way inseparably associated with the disease. Those who dissent from the idea of its being the *causa causans* prefer to class the comma-bacillus itself as the harmless stage in the life history of the organism which at an earlier or later stage constitutes the materies morbi. So far as he knew, there is no instance on record of the transformation of one kind of bacteria into another, and in so brief an existence as that of the comma-bacillus, where the stages in its life history must necessarily follow each other in rapid succession, there could not be one pathogenic stage

while all the rest are harmless. It is true that some bacteria—for example, anthrax bacilli—lose their pathogenic properties when subjected to the influence of certain reagents, but a change of form has never been noticed.

The above, however, is an illustration diametrically opposed in character to the one of which we are speaking. Here we would have a harmless intestinal bacterium take on instead of throw off its pathogenic characteristics. Another point not to be overlooked in this connection is that the proportion of comma-bacilli to other intestinal bacteria has been rated as high as ten to one, which would imply that during its brief stage of harmlessness its power to multiply is increased tenfold. This, of course, is contrary to every known law.

Another theory is that the comma-bacillus is the scavenger rather than the cause of the disease—that is, conditions are created by the disease through which, among the many bacteria to be found in the intestine, one kind or another is changed and assumes the qualities observed in the comma-bacillus. The explanation given in the above case is equally applicable here. Our knowledge of bacteria shows us that, so far as form is concerned, they are all remarkably constant. If they were not, successive cultivations outside of the body would cause them sooner or later to revert to their original form. The comma-bacillus has been carried as high as the twentieth cultivation in various nutrient media, and yet no alteration either in form or manner of growth has been observed. If it were only a transitional stage of some bacterium, that fact would certainly be revealed during successive cultivations. Likewise, if it is the scavenger of the cholera process, then as soon as it is separated from the cholera process the germs would cease to flourish. But it is constant throughout.

Klein expresses the view that the cholera process favors the growth of comma-bacilli by preparing a nutritive soil for them, and on this hypothesis he endeavors to explain the striking increase of this particular kind of bacteria. If this is so, then we must start with the understanding that the comma-bacillus exists in the perfectly healthy body; but the most searching investigations have failed to reveal the slightest trace of an organism that in any way resembles it either in form or manner of growth. Moreover, in cases which eventually terminated in recovery, there was manifestly a decrease in the number of comma-bacilli corresponding to the stage of convalescence.

Of course, if it were possible to induce the choleraic condition in animals by the inoculation of comma-bacilli, there would be no room for controversy as to the causative power of this organism. Even if by feeding appreciable quantities of this form of bacteria any derangement of the digestive process were brought about, it would at least give encouragement as to its causative properties. So far from having either of these effects, its action is not perceptible. This seems a strong barrier, and were it possible to bring to account the case of a single animal fallen ill with the disease during any of the epidemics, it would certainly be an insurmountable obstacle to the consideration of the causative relation of the comma-bacillus. But from all the quarters that have been visited by epidemics come unanimous reports as to the exemption of animals from a condition in any way resembling cholera. This carries with it a good deal of weight, which alleviates in a degree the disappointment resulting from the failure of inoculation experiments. It was evident, from careful examination of the intestines, that the comma-bacilli had been killed during their passage through the stomach; in fact, few were found in the intestinal canal, being confined, for the most part, to the walls of the stomach. It is probably true, too, that if the digestive tract of man is free from derangement, he will likewise be able to resist the disease.

While under Koch's tuition at Berlin in the winter of 1884, Dr. Lewis busied himself particularly in the line of inoculation experiments, being quite as sceptical as any one could be in regard to the claims made for the comma-bacillus. The animals used for this purpose were white mice, guinea-pigs, and rabbits. No effect whatever was produced by feeding a pure culture, but when the bacilli were introduced directly into the duodenum, the guinea-pigs and rabbits, some six hours later, manifested most of the symptoms of cholera infection. The mice, however, exhibited no trustworthy symptoms, dying apparently from indisposition. Upon examination of the intestines after death, large numbers of cholera bacilli were found in the region of Peyer's glands, the glands themselves being enlarged and inflamed. These appearances were very distinct in the guinea-pigs and rabbits, but scarcely perceptible in the mice. From a physiological standpoint, there is no apparent reason why the mice should be affected differently from the guinea-pigs and rabbits, and yet throughout Germany experiments upon mice have been unsatisfactory. The small size of the animal, and the consequent care that must be taken in reaching a desirable point of inoculation, will explain in part the failure in a large number of cases. This is evident from the fact that a great many of the mice die from peritonitis rather than from any pathological effect produced by the bacteria.

If we assume that a disease is caused by a specific germ, we cannot think of an autochthonous origin, emanating from any particular locality. Such a specific organism must follow the laws of vegetation, just as the most highly developed plant. It must always propagate itself from something of the same nature, and cannot spring up at haphazard from other things or from nothing. In the case of the comma-bacillus, the home of which has been quite clearly defined, we are forced to trace back the disease that depends upon it to special localities from which this specific microörganism is brought to us.

MR. EUGENE RAU read

AN ACCOUNT OF MICROSCOPICAL EXAMINATION MADE IN TWO FATAL CASES OF TRICHINOSIS IN BETHLEHEM, PA.

On February 8, 1886, samples of pork were submitted to him for examination, the use of which, it was supposed, caused the illness of a family consisting of man and wife and two daughters, five and thirteen years of age. The pork was found to contain trichinæ varying in numbers from two to six, and occasionally as many as eight in a field view. The meat had been freely used for a week from January 6th. An $\frac{8}{10}$ objective with an A eye-piece was used on the microscope for all the examinations made; the field view being about $\frac{1}{12}$ of an inch in diameter.

The daughter, aged thirteen, died after an illness of

five weeks, and an examination of a small portion of the deltoid muscle revealed trichinæ to the number of twelve to eighteen, and in one instance as many as forty-two were counted in the field view. The results of these examinations conclusively show the cause of the disease; no other portions were examined.

In the mother, aged thirty-seven, who died after an illness of nearly eight weeks, the deltoid muscle was found to contain three to nine, the rectus femoris two to six, and the diaphragm one to three trichinæ to a field view. The lungs, heart, liver, spleen, and kidneys were not found to be infected.

The pork used was home-raised, and, according to the owner, the animal did not at any time show signs of being unwell. An examination of two other hogs raised on the premises was made, but no trichinæ were found. For several days, while in water, the human trichinæ showed signs of life, coiling and partly uncoiling when freed from the muscle fibre. Those in the pork being in the encysted condition, showed no activity when separated and placed in the same conditions as the former.

The trichinæ in the deltoid of the daughter were found free in the muscular fibres, while those in the muscles of the mother were encysted. Eight photographs were submitted; all with the exception of one were made with a $\frac{2}{3}$ objective without an eye-piece. Four are from the human subject and four from the pork. The length of the trichina uncoiled was $\frac{1}{80}$th and diameter $\frac{1}{700}$th of an inch.

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

Annual Meeting, held at Buffalo, August, 1886.

(Specially reported for THE MEDICAL NEWS.)

THE Annual Meeting of the American Association was held at Buffalo this year, and we present abstracts of the papers of medical interest which were read.

DR. BURT G. WILDER, of Ithaca, read a paper on

HUMAN CEREBRAL FISSURES; THEIR RELATIONS AND NAMES, AND THE METHODS OF STUDYING THEM.

It embodied the results of a study of the subject which has been wholly interrupted since 1873, when a paper on the fissures of the carnivora was read before the Association. He believed that, for the present, the attempt to refer the human fissures to a series of arches like those of some carnivora, has not been of use to students, and that the beginner must learn their relative positions and names as a task for the memory alone.

It is easier, however, to treat the cerebral surfaces as regions of an unexplored country, to determine directions and landmarks, and to learn certain fissures before others. The landmarks are the callosum, the Sylvian fissure, and the margin of the hemicerebrum. The first fissures to be learned are the interlobar, then the constant, intergyral, with ental correlatives, then those with no such correlatives; finally, the inconstant, intragyral fissures. He enumerated forty-six fissures, more than have been designated by any previous writer.

His views as to the relations of the fissures were mostly indicated by diagrams with the names thereon. The latter are mostly mononyms, selected or formed from names in common use. The following fissures were discussed in detail: presylvian; exoccipital; lambdoidal; paracentral; paroccipital; meditemporal; precentral; post-central; adoccipital; intermedia; subsylvian. The gyral names were discussed briefly.

In conclusion, he urged that, in contravention of the view and practice of Gratiolet, Meynert, and some others, the brains of monkeys be disregarded as preliminary to the human, and that fœtal brains be preserved and carefully studied.

DR. C. S. MINOT, of Boston, then read a paper on

HOMOLOGUES OF SEGMENTATION OF THE OVUM IN VERTEBRATES.

The ovum divides first into two sets of cells, the small ectoderm and the large entoderm cells. The line of junction between the two is the ectoental line, part of which forms by concrescence the primitive streak, and its homologues. The ectoderm is primitively many layered, and becomes one layered later. The inner cell mass of the mammalian ovum is the true ectoderm, while the supposed ectoderm of the mammalian blastodermic vesicle is really the entoderm. The mesoderm grows out from the ectoental line. The entodermic cavity is always distinct from the segmentation cavity, but the view hitherto prevalent in regard to the homologies of the mammalian blastodermic vesicle, make the two cavities identical. All the variations can be correlated with the greater or less development of the yolk.

PROF. J. H. PILLSBURY, of Smith College, Northampton, Mass., read a paper on

AREAS OF FORM AND COLOR PERCEPTION IN THE HUMAN RETINA.

After describing the methods of observation and apparatus used, the paper discusses the following points.

1. Relation between myopia of the eye and the extent of the area of form vision.

2. Relative extent of areas of form and color perception.

3. Relative extent of areas of color perception for different colors.

4. Some curious irregularities of areas of color perception.

DR. C. S. MINOT, of Boston, offered some remarks

ON THE DEVELOPMENT OF THE HUMAN CHORION.

The human chorion comprises at the third week of gestation two layers: 1, a mesodermic layer consisting of rounded cells embedded in a glistening matrix; 2, an ectodermal pseudoepithelium. The first changes, during the second month, into spindle-celled connective tissue with large, round, granular cells of unknown origin scattered through it. The second undergoes a complicated metamorphosis. The cells proliferate in numerous patches forming several layers; the patches enlarge and fuse to a greater or less extent. The basal layers of the thickened portions give rise to Langhan's cellular layer, and the outer parts undergo a remarkable hyaline degeneration to produce Langhan's fibrin layer. The chorionic epithelium is, therefore, not lost, but only metamorphosed.

DR. H. P. BOWDITCH of Boston, made some remarks on the

VASOMOTOR NERVES OF THE LIMBS.

In most of the recent experiments on the vasomotor nerves of the limbs, the temperature of the skin has

been taken as the index of vasomotor activity. It is evident, however, that by this method transitory changes cannot be recorded and quantitative results cannot be obtained. In experiments made with the plethysmographic method, to which these objections do not apply, it was found that either a constriction, or a constriction followed by a dilatation, or a dilatation of the vessels of the leg may be produced by an electric stimulation of the sciatic nerve. The character of the result depends largely upon the strength and rapidity of the induction shocks used as a stimulus. In the following table are given the results of 909 observations of the effect of stimulations of the sciatic nerve, with induction shocks of varying intensity and rate. The figures in the columns headed —, — +, and + show in what per cent. of the total number of observations the effect of the stimulation of each rate and strength was respectively a constriction, a constriction followed by a dilatation, and a dilatation.

Rate of Stimulation.	Weak.				Medium.				Strong.			
	No. of observations	Per cent.			No. of observations	Per cent.			No. of observations	Per cent.		
		—	— +	+		—	— +	+		—	— +	+
0 – 4 in 1″	108	9.3	57.4	33.3	218	7.8	69.7	22.5	142	6.3	69.7	23.9
5 – 16 in 1″	69	31.9	60.9	7.2	153	28.8	58.8	12.4	109	5.5	89.9	4 6
30 – 64 in 1″	62	48.4	50.0	1.6	40	50.0	50.0	0.0	8	37.5	62.5	0.0

It will be observed that with each rate and intensity of stimulation, the result, in at least one-half of the observations, is a constriction, followed by a dilatation; (2) that with an increasing rate of stimulation, the proportion of cases giving a simple dilatation diminishes, while (except with strong stimulations) that of cases giving a simple constriction increases.

With slow irritations it was found that dilatations are more readily produced with feeble than with strong irritations. Experiments with animals whose nerves had been cut several days previously showed that degeneration of the nerve caused the loss of its constricting earlier than that of its dilating power.

It thus appears that the sciatic nerve contains fibres which both constrict and dilate the vessels, but the former effect being more prompt than the latter, both in its appearance and disappearance, the two effects never neutralize each other.

A fresh nerve with a strong and rapid stimulation, offers the best conditions for producing constriction. A degenerate nerve, with a feeble and slow stimulation, offers the best conditions for producing dilatation.

(To be continued.)

CORRESPONDENCE.

THE OBSTETRICAL SECTION OF THE BRITISH MEDICAL ASSOCIATION.

THE meetings of the British Medical Association, at Brighton, have just terminated with the Mayor's ball on Friday evening, and a temperance breakfast early Saturday morning, at which at least one hundred and fifty members were present. Throughout the week the city was crowded with medical men from all over the English-speaking world.

The addresses, made in the mornings before the whole Association, by Withers Moore, M.D., F.R.C.P., Major J. S. Billings, M.D., F. A. Humphrey, F.R.C.S., and E. D. Mapother, M.D., were equal to any the Association has listened to in former years, while that of Dr. Billings in particular was both instructive in the highest degree, and full of the happiest hits, never allowing the interest of the audience to flag for a moment. The afternoons were devoted to the meetings of the Sections.

Your correspondent's attention was confined almost exclusively to the work of the Obstetrical Branch, with the exception of the addresses made upon the subject of the "Surgery of the Liver," before the Surgical Section, on Thursday afternoon, by George Harley, M.D., F.R.S., J. K. Thornton, Esq., A. Willett, Esq., and Lawson Tait, Esq. These latter communications took the form of distinct papers, detailing the original work of the authors, and they touched upon many widely diverse points. Dr. Harley related cases of acute hepatitis in which he had, with perfect results, punctured the liver with a large trocar about six inches in length, and, as in one instance, drawn off twenty ounces of blood. He also recommended multiple punctures of the capsule in hypertrophic cirrhosis, which he had also found a valuable remedial expedient. Mr. Thornton brought before the Society all the cases in which he had found it necessary to interfere with the liver structure in his abdominal sections. His conclusion was that the liver was far more tolerant of injury than is generally believed by surgeons.

Mr. Willett related a case of cholecystotomy. Mr. Tait followed with a brief address, the text for which was furnished by three tables including the fifty cases in which he had operated upon the liver. The tables were in the hands of the members in the form of proof-sheets. Seven of these cases are set down as "exploratory incision," two of which revealed "cancer," one of the liver, and one of the gall-bladder; three are labelled "tumor," one "hepatic colic," and one "miliary abscess." One "tumor" died of colloid cancer. Following these are "thirteen cases of hepatotomy," all recovered; nine of them were performed for "hydatids of liver," two for "cyst of liver," one for "tumor of liver," and one for "gall-stones." The youngest patient on the list, age seven years, is found here, for "hydatids of liver." The table concludes with "thirty cases of cholecystotomy," twenty-six of which were "gall-stones," with one death. One was a case of "cholœmia," and two of "distended gall-bladder," and one of "suppurating gall-bladder."

The whole table is suggestive, and capable of some

interesting analyses. More than half the operations were undertaken for gall-stones. The average age of those operated upon was nearly forty-five years (44.8), and more than forty per cent. (40.7) were in the forties, while nearly sixty-three per cent. (62.9) were between thirty-five and fifty-five years of age.

The ages of the cases operated upon for hydatids differ more widely than those of any other class, but the average is only thirty-one years, the extremes being seven and sixty-three.

Dr. Alfred Meadows, President of the Obstetrical Section, delivered the opening address on Wednesday afternoon at two o'clock. He first spoke of the tendency in England, Germany, and America to divorce obstetrics and gynecology, and then eulogized the present status of our science, referring more in detail to some of the most recent achievements. The reaction against craniotomy in Great Britain seems to be of the strongest nature. Dr. Meadows spoke of it as an operation "barbarous and repulsive to every religious sentiment," while the Porro method is a distinct advance in the direction of saving life. He quoted Tait's dogma, that in the latter operation there was at least a chance of saving the life of the child, while the dangers of craniotomy were as great to the mother as in Porro's operation. To this frightful operation —craniotomy—nearly two thousand lives and some hundreds of mothers are sacrificed in England yearly.

Dr. Meadows proceeded to dwell most ably upon the moral and scientific bearings of the case, and urged the fact upon the members that here, in this particular field, must we look for some of the greatest advances in the future.

Removal of the *uterine appendages* is an erroneous term, as the *uterus* is the appendage to the organs in question. The startling statistics of this most recent innovation in gynecological surgery call for comment. The subject has important moral bearings, and like all else encroaching upon the moral world, is apt to provoke angry discussion. It clearly has its legitimate field in certain cases of, by other means incurable, nervous disease, in cases of rapidly growing fibroids, and, without question, in distinctly developed ovarian and tubal disease. The operation ought also, in cases where birth *per vias naturales* cannot be accomplished, to take the place of the Porro operation and craniotomy and the many disgusting methods recommended to prevent conception. We must in all these cases aim at greater diagnostic precision, and not be too lavish in our selection of those deemed fit for operative interference.

Dr. Robert Barnes read the first paper on the subject "Alternatives to Craniotomy." This operation was so frequent in Tyler Smith's time, that it numbered one in every three hundred and forty labors, and at one time the perforator was more frequently used than the forceps. The deformities which call for craniotomy arise for the most part in childhood, and, as has been shown in one English town, are to be prevented by hygienic measures, and anti-syphilitic precautions.

Next, after hygiene, comes the induction of labor, either before the viability of the child, or during the last three months; and from the thirty-sixth to the fortieth weeks we have often the alternative of turning. When the patient has arrived at full term with a living child which cannot be brought alive through the pelvis, the alternatives are two, craniotomy or Cæsarean section. In extreme cases of pelvic deformity the latter, or some kindred method, becomes a necessity, but in pelvic contraction of about three inches embryotomy ought not to give a greater mortality than ordinary labor, and, until the chances from the Cæsarean section equal those of embryotomy, the question cannot be considered as solved in favor of the former, which abandons the old moral law that the mother's life is more important than that of the unborn infant. Will the Cæsarean section be improved up to this point? In from three to three and a quarter inch deformed pelves perform craniotomy, and in future induce premature labor. This measure will only be abolished with the triumph of hygiene.

Dr. Kinkead, of the Royal Infirmary of Dublin, ably considered the question, "Can the maternal mortality of the Cæsarean section be reduced to that of craniotomy?" The difficulties in the way of an accurate comparison are great. Consistently with the showing of Parry that in cases of great deformity (two and a half inches) the maternal mortality of the two operations are equal, we should choose the sectional method. The mortality of the Cæsarean section is great, but it is due to the delays, and the blame for this must fall on those teachers who ever assert that it is an operation of almost necessary fatality. In those cases in which no other attempts to deliver have been made, and the operation has been undertaken within the first twenty-four hours, Dr. R. P. Harris has shown that the maternal mortality of the Cæsarean method is but twenty-five per cent., while all tables of craniotomy in two and a half inch pelves show a mortality of from thirty-seven and a half per cent. (Parry) to seventy-five per cent. in carcinoma of the uterus. Shock, hemorrhage, peritonitis, and incarceration of the intestine are the prime factors in causing death after the Cæsarean method. The first is relieved by early operation, the second by elastic ligature, and the second and third and fourth by close approximation of the wounded surfaces.

In the course of the discussion which followed these two papers, Dr. Lusk, of New York, made a very clear statement of the present status of the subject. Deformities below three inches are rare in New York. That method is a welcome one which will relieve us of the horrible operation—craniotomy—which he has often had occasion to perform.

It is a fact that for one hundred years no successful Cæsarean section was performed in the Vienna and Paris hospitals; but what ought we to expect when the natural death-rate at the time was between five and ten per cent? Dr. Harris has shown us that under favorable circumstances between seventy-five and seventy-six per cent. recover, and that all fatal cases result from conditions making the issue almost inevitable. Michælis reports cases in which the woman was operated upon with a razor, another in which the patient was dying, and still another in which the patient was actually a corpse, and yet these are attributed to the natural mortality of the operation.

Porro's operation was at first adopted so widely because it was believed that the wound would, of necessity, gape in the Cæsarean operation. Sänger, however, disproved this, and taught us the conditions of success, reporting four cases without a death. A recent American

operator gave as his reason for choosing Sänger's method, in place of laparo-elytrotomy, for which his case was best adapted, that he did not wish to burden this latter with another death.

These various methods are not rivals, but clearly fulfil different indications. The Cæsarean method must have the preference in favorable cases taken early. The Porro method is best adapted to cases in which the uterus is exhausted, and the presumption is that degenerative changes have occurred. Laparo-elytrotomy is the operation for cases in which, with the head at the brim, the uterus has retracted above it.

Dr. Wilson, of Baltimore, stated that he had not performed craniotomy for thirty years, and agreed with Dr. Lusk as to the various fields he had indicated for these different methods.

Lawson Tait spoke in favor of the Porro method, and thinks we are bound to protect the woman from the future dangers from her two and a-half inch pelvis. Half of the children die anyhow, so why should we argue against an operation which prevents childbearing? Craniotomy too, is just as fatal as the Cæsareon section.

Dr. T. M. Madden, of Dublin, speaking from an experience of twenty years, in the largest lying-in hospital in Great Britain, said that he had never performed craniotomy, and he looked at the question from a moral standpoint; the balance of life should not be poised on the hand of the physician as here. It is but too often as Simpson has said, "Cold steel for the child, and mercury for the mother," to combat the peritonitis.

Dr. T. A. Emmet, of New York, read a paper on "Certain moot Points in Gynecology," and was immediately followed by Dr. Grailey Hewitt on "The Early History and Etiology of Uterine Flexures and Displacements."

Dr. Emmet made a very clear, succinct statement of his views, declaring that versions are but symptoms, and stenosis is a matter of little importance. The relief following the use of a pessary is not due to the correction of a displacement, but to the fact that it takes up the slack in the broad ligaments, and supports the blood-vessels, relieving the congestion. The common symptom of irritable bladder is not due, as so often supposed, to a prolapse of the uterus, but to the traction by the cervix on the utero-vesical folds, and thus, by lifting the cervix with a pessary, relief is obtained. Flexure is often merely an individual habit. Dr. Emmet has observed the most extreme stenoses without accompanying symptoms.

Dr. Emmet recorded a case seen lately, in which the cervix had been divulsed two years ago, and she is now under his treatment for a pelvic peritonitis, from which she has suffered ever since, and he has observed a rapidly growing epithelioma.

If there is one thing his experience in the past fifteen years has taught him, it is this constant association between lacerated cervix and epithelioma. Pelvic inflammation is best treated by small doses of ergot and the hot water douche. Dr. Emmet has not owned a sound for three years and possesses no uterine probe. An analysis of the cases under treatment before and after giving up intrauterine applications, shows a most remarkable difference, in favor of the latter, in the average number of days the cases remained under treatment.

Dr. W. T. Lusk read a paper on Thursday afternoon on "The Proper Moment for the Performance of Gastrotomy in Abdominal Pregnancy." This was followed by Dr. T. H. Aveling on "A Case of Extrauterine Gestation Arrested by Electricity."

Dr. Michael Beverley reported twenty cases of Emmet's operation for "Laceration of the Cervix;" and three for "Procidentia Uteri." The record was an admirable one, showing the clearest insight, on the part of Dr. Beverley, into the etiological factors concerned. In the discussion following, Dr. Kelly pointed out that lacerated cervices were not any more frequent in America than in England and Germany, the sole difference was one of *names* only; all those cases which abroad were labelled and treated as *erosions*, were almost without an exception *lacerations*. And no better illustration of this could be found than in the excellent colored picture in Ruge and Veit's "Erosio Uteri und beginnender Krebs," published in the *Zeitschrift für Gynäkologie*. Here the pictures so generally copied as types of erosions, were typical examples of as many grades of lacerations, the first two requiring puncture and packing or the douche to soften them down, bring the lips together and, by the rational plastic operation, complete the cure. Dr. Kelly also drew attention to the three essential points to be borne in mind in all operations practised on the vagina with a view of retaining the pelvic viscera.

The Section closed with a paper read on Friday morning by Dr. Thomas Savage, of Birmingham, on "Removal of the Uterine Appendages," followed by a timely protest by Dr. Moore Madden upon the same subject. Dr. Savage expressed himself well, and to the satisfaction of the members, who concurred in his views, almost without an exception. The discussion following was of unusual interest, and was participated in by Dr. G. Bantock, of London; Dr. E. T. Davies and Dr. Elder, of Nottingham; Dr. Wilson, of Baltimore; Dr. Imlach, of Liverpool; Dr. Gordon, of Maine; Dr. Gill Wylie, of New York; Mr. Lawson Tait, of Birmingham; and Dr. H. A. Kelly, of Philadelphia. Dr. Wylie's remarks added especial interest, as he detailed some of his good work in dealing with difficult tubal and ovarian cases in New York.

On Thursday Dr. G. Ranney, of Michigan, read an interesting paper upon the treatment of mastitis by compression.

It is impossible to conclude without acknowledging the constant kindness and courtesy the distinguished President and members of the Society showed their visitors, which only ended with a handsome lunch given by Dr. Meadows at the Grand Hotel at the close of the final meeting. HOWARD A. KELLY, M.D.

BIRMINGHAM, August 20, 1886.

CATGUT SUTURES FOR OPERATIONS UPON THE LACERATED CERVIX AND ELSEWHERE.

To the Editor of THE MEDICAL NEWS,

SIR: Dr. Robert T. Morris makes a little hit at Dr. Sutton in your issue of July 17th, and makes at the same time a rather sweeping assertion about gynecologists generally which I cannot permit to go abroad unrefuted. Undoubtedly the surgeons who "insert sutures of antiseptically prepared catgut into the lacerated cervix, sprinkle the surface with iodoform, and do not disturb

the womb with frequent washings," are doing the proper thing, and a very clever thing too. But gynecologists, especially abroad, do the same, and he should not make so unqualified a statement representing this as the invariable habit of general surgeons, and only of surgeons, and that gynecologists are absolutely in the dark regarding these simple methods of treatment. Let me say that for many years, certainly five or six years, I have pursued the course he speaks of, and which Dr. Sutton now advocates, never using washes or injections in the after-treatment of uterine or vaginal operations. I have treated all such wounds by a strictly dry antiseptic method—that is, a let-alone method; after an operation antiseptically performed I have dusted the surface with iodoform, covered it with antiseptic cotton or gauze, and so left it until, when silk was used, it was time to remove the sutures, or when catgut was used, I had reason to change the dressing perhaps on the fourth or fifth day, perhaps not until I desired to examine the part. Cleansing of the surface by vaginal injections, washes, etc., I have never insisted upon, and strictly forbidden since antisepsis has been taught.

During the past year I have used catgut to the exclusion of every other material for all operations upon the uterus, vagina, and perineum, and, in part, even for the closing of abdominal incisions. For the past ten years it has been my wish to use it, knowing that theoretically it was the correct thing; but the catgut, as offered for sale, prepared in various ways, was neither a convenient nor agreeable material for use. I always had it on hand, often attempted to use it, but finally gave it up in disgust. I have tried whatever has been recommended: in early years I clung to the silver wire, then to silk, which I have used for the last four or five years, in preference to silkworm gut, kangaroo tail, or whale tendon, which I have tried without discovering any great advantages; I have used silk exclusively, giving other material a fair trial; but since I have seen the catgut prepared with corrosive sublimate, glycerine, and alcohol, I have used nothing else, and I fully agree with the high estimate which Dr. Morris places upon this material, and which is, notwithstanding his assertion, well known to gynecologists, and used almost exclusively by some.

Catgut so prepared is undoubtedly superior to any other material: *first*, it is very strong, almost as strong as silver; *second*, it is less irritating than any other material; *third*, it is as convenient and agreeable to handle as silk; and *fourth*, it is self-removing, and can be timed with almost the exactness of a fuse; by selecting the proper size we can control the time during which it shall bind the parts. It is of the greatest advantage to use a material so nearly allied to the tissues which it holds, and which we are not obliged to watch, lest we run the risk of leaving a suture, and thus keeping an irritant, a foreign body in the parts.

The dry method, which I have for many years adopted for the dressing of recently united parts, is the only one which is really antiseptic, if not aseptic, and this is the only proper method of dressing in the after-treatment after antiseptic operations. Some gynecologists, both in France and Germany, use catgut freely; but I believe no one has used it so exclusively as I have, relying upon it for perineal, and even abdominal operations.

Truly yours,

ST. LOUIS, August, 1886. GEORGE J. ENGELMANN.

NEWS ITEMS.

AMERICAN SOCIAL SCIENCE ASSOCIATION.—The General Meeting of this Association for 1886 will be held at Saratoga, N. Y., from September 6th to 10th, inclusive, opening at 8 P. M., September 6th, with an address by the President, Carroll D. Wright, of Boston. The Department of Education will meet on Tuesday, September 7th; the Health Department on Wednesday, September 8th; the Department of Jurisprudence on Thursday, September 9th; and the Social Economy Department on Friday, September 10th. Papers on scientific subjects will be read by Drs. Grace Peckham, Holbrook Curtis, Valentine Mott, Wallace Wood, T. Munson Coan, all of New York.

THE PASTEUR EXHIBIT AT THE EXPOSITION OF URBAN HYGIENE AT PARIS.—Men and women gape wonderstruck before a table loaded with a hundred or so glass mattrasses in which Pasteur works his *bouillons*. A large sign cautions the public, "Do not touch: very dangerous;" and the concoctions of charbon, hydrophobia, typhoid, and other uncanny microbes are severely left alone. The cages in which the unfortunate rabbits are kept are only seen in photographs, but rabid marrow and dog-rolling cages are there in reality. The latter are made of strong galvanized iron, and provided with rollers, a hook on top, and a perforated bottom. In each cage remained the tin dish used for the prisoner's food; the metal's edges are deeply indented and torn in some places by the teeth of the late occupants, showing but too vividly the horrible disease that carried them off.

PASTEUR'S METHOD.—In the House of Commons lately Charles Thomson Ritchie, President of the Local Government Board, stated that the committee appointed by the government to examine into the Pasteur method of inoculation for rabies was fully satisfied that the French savant's treatment for hydrophobia was effective.

INTERNATIONAL STATISTICS OF THE INSANE.—The conference of the various committees appointed by scientific bodies and eminent alienists, to agree upon a uniform classification of mental diseases, in order to secure the best basis of International statistics of the insane, in coöperation with the International Committee appointed at the Antwerp Congress, held by the Belgian Society of Mental Medicine last autumn, will be held at Saratoga, N. Y., on September 8, 1886, at 10 o'clock, A.M. Dr. Pliny Earle has been invited to preside.

COLDWATER SANITARY CONVENTION.—The time for holding the Sanitary Convention at Coldwater, under the auspices of the Michigan State Board of Health, has been changed from September 23 and 24, to September 9 and 10, 1886.

THE ENGLISH CONGRESS OF HYGIENE will be held at York in September, under the presidency of Sir Spencer Wells.

THE FIFTY-NINTH REUNION OF GERMAN NATURALISTS AND PHYSICIANS will be held in Berlin from the 18th to the 24th of September. A cordial invitation has been isued to those interested in all countries.

HONORS TO MEDICAL MEN.—The Queen has recently conferred the honor of knighthood upon the following gentlemen amongst others: Mr. John Tomes, F.R.C.S.; Mr. Douglas Maclagan, M.D.; Surgeon-General Thomas Longmore, C.B., Professor of Military Surgery at Netley; and Mr. Edward Sieveking, M.D., Physician Extraordinary to the Queen. On the 2d inst., amongst the distinguished colonial visitors who were invested with various Orders, the following were made Companions of the Order of the Indian Empire: George Watt, M.D., and John W. Tyler, M.D.

SUBAQUEOUS VEGETATION.—An interesting discovery has been made in Switzerland of a bright green moss, growing on calcareous rock two hundred feet below the surface of Lake Leman. No other moss has been known so far under water; and how chlorophyl, the green coloring-matter, could have been so richly developed in a place so remote from the light, is a problem; probably the extraordinary clearness of the water enabling the sun's rays to penetrate to a great depth.

THE SCIENTIFIC SPIRIT.—Let those who think they know, without ever having looked to see, review their supposed knowledge and cast their thoughts over again; and if, in the particulars, they find they have mistaken words and fancies for realities, and accepted the dicta of pretenders instead of the evidence of observed facts, let them correct the record, and acknowledge the truth as it is in nature. Moreover, let them remember that he who propagates a delusion, and he who connives at one when already existing, both alike tamper with the truth, and that we must neither lead nor leave men to mistake falsehood for truth. Not to undeceive is to deceive.—DR. T. WHARTON JONES.

THE USE OF GAS COOKING-STOVES is increasing in Great Britain. Many of the Scottish gas companies now let out the stoves at a cheap rate. Dr. Stevenson Macadam speaking of gas-cooking in its sanitary aspects, says: "The wholesomeness of the meat cooked in the gas-stoves must be regarded as beyond doubt. Gas-cooked meat will be found to be more juicy and palatable, and yet free from those alkaloidal bodies produced during the confined cooking of meat, which are more or less hurtful, and even poisonous." A joint cooked in a gas-oven weighs heavier than the same joint cooked in a coal-oven, because the juices are more perfectly preserved in it.

MEDICAL STUDENTS IN SWISS UNIVERSITIES.—The number of medical students in Switzerland appears to be increasing. During the present summer session the total is 675, including 81 women, who are admitted in Berne, Geneva, Zurich, and the preparatory school of Lausanne, but not in Basle. In Berne there are no less than 40 female students; some of the assistants' posts are filled by women. It is remarked that their exercises, or degree theses, are usually selected from theoretical subjects—such as anatomy, physiology, pathological anatomy, and pharmacology—rather than from practical medicine and surgery. The increase in the number of sudents is due almost entirely to an accession from Switzerland itself, the number of foreigners (192) being much the same as in former years. Of the 81 female students, only 10 are of Swiss nationality.

NOTES AND QUERIES.

GUTTA-PERCHA FOR MARKING THERMOMETERS, URINOMETERS, ETC.

For marking thermometers and urinometers that have become indistinct from wear and washing in the one case, and the action of urine in the other, I take an old hard-rubber thermometer case, hold one end in a gas flame until it ignites, and then rub the burning end over the figures, running all the grooves full of melted gutta-percha. After waiting a few minutes for it to cool, I take a sharp knife and cut off the surplus rubber, and leave the grooves filled with black, shiny, insoluble gutta-percha. To mark bone-handled instruments, I scratch, or cut, whatever mark I wish to have on them, and use the gutta-percha as described above. An old gutta-percha button answers the same purpose as a hard-rubber thermometer case.

J. NEELY RHOADS, M.D.,
Resident Physician
Jefferson Medical College Hospital.

Philadelphia, August 31, 1886.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT, U. S. ARMY, FROM AUGUST 24 TO AUGUST 30, 1886.

WOODHULL, A. A., *Major and Surgeon.*—Granted leave of absence for fourteen days, to take effect on or about September 1, 1886.—*S. O. 197, A. G. O.*, August 25, 1886.

TAYLOR, M. K., *Major and Surgeon.*—Leave extended one month.—*S. O. 195, A. G. O.*, August 23, 1886.

LAUDERDALE, J. V., *Captain and Assistant Surgeon.*—Granted leave of absence for two months, with permission to apply for one month's extension, when his services can be spared.—*S. O. 195, A. G. O.*, August 23, 1886.

GANDY, CHAS. M., *First Lieutenant and Assistant Surgeon.*—On expiration of his present leave of absence, relieved from duty in the Department of the East and assigned to duty in the Department of Texas.—*S. O. 195, A. G. O.*, August 23, 1886.

OFFICIAL LIST OF CHANGES IN THE MEDICAL CORPS OF THE U. S. NAVY, FOR THE WEEK ENDING AUGUST 28, 1886.

CLARK, JOHN H., *Medical Inspector.*—Ordered to special duty, Portsmouth, N. H., and Widow's Island.

KINDLEBERGER, D., *Medical Inspector.*—Ordered to Hospital, Washington, D. C., October 1, 1886.

GIHON, A. L., *Medical Director.*—Ordered to Hospital, Mare Island, Cal., October 15, 1886.

ROBINSON, SOMERSET, *Medical Inspector.*—Detached from Hospital, Mare Island, Cal., October 15, 1886, and wait orders.

SPEAR, J. C., *Medical Inspector.*—Ordered to Naval Laboratory, New York, September 28, 1886.

BLOODGOOD, DELEVAN, *Medical Director.*—Ordered to Hospital, Norfolk, Va., September 29, 1886.

TAYLOR, J. Y., *Medical Director.*—Ordered to Naval Laboratory, New York, September 29, 1886.

DEAN, R. C., *Medical Director.*—Detached from Naval Hospital, New York, and wait orders.

SIMON, W. J., *Surgeon.*—Detached from U. S. S. "Constellation," and ordered to special duty at Naval Academy, Annapolis, Md.

HENRY, C. P., *Assistant Surgeon.*—Ordered to Hospital, Philadelphia, Pa.

FITTS, H. B., *Passed Assistant Surgeon.*—Detached from Hospital, Philadelphia, Pa., and wait orders.

STONE, *Assistant Surgeon.*—Ordered to Receiving Ship "New Hampshire."

THE MEDICAL NEWS *will be pleased to receive early intelligence of local events of general medical interest, or of matters which it is desirable to bring to the notice of the profession.*

Local papers containing reports or news items should be marked.

Letters, whether written for publication or private information, must be authenticated by the names and addresses of their writers—of course, not necessarily for publication.

All communications relating to the editorial department of the NEWS *should be addressed to No. 1004 Walnut Street, Philadelphia.*